Sociobiology Packet

A set of articles assembled by the Sociobiology Study Group of Boston Science for the People around 1979

Science for the People

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SOCIOBIOLOGY--A NEW BIOLOGICAL DETERMINISM

by the

Sociobiology Study Group
Science for the People

Sociobiology—A New Biological Determinism

Sociobiology Study Group

Biological determinism attempts to show that the present states of human societies are the result of biological forces and the biological "nature" of the human species. Deterministic theories differ in detail, but all have a similar form. They begin by describing the particular model of society that corresponds to the author's socioeconomic prejudices. It is then asserted that the characteristics of that society are a necessary consequence of the biological nature of the human species. Therefore, present human social arrangements are either unchangeable or, when altered, will demand continued conscious social control because the changed conditions will be "unnatural." Moreover, such determinism provides a justification for the *status quo*, although some determinists dissociate themselves from the consequences of their arguments. The issue, however, is not the motivations of individual creators of deterministic theories but the ways in which those theories operate as powerful forms of legitimation of past and present social institutions. (See Bookchin's article in this volume.)

It is not surprising that the model of society that turns out to be "natural" bears a remarkable resemblance to the institutions of modern market society, since the theorists who produce these models are themselves privileged members of just such a society. Thus, we find that aggression, competition, extreme division of labor, the nuclear family, the domination of women by men, the defense of national territory, and individualism are over and over stated to be manifestations of "human nature."

The precise form of biological determinism has varied, depending upon the particular social institutions to be justified and upon the state of biological science at the time. In the nineteenth century, when genetics had not yet been invented and biological evolution was still controversial, zoologists like Louis Agassiz, an anti-evolutionist, tried to prove the biological inferiority of races and classes by the evidence of comparative anatomy and embryology. On the other hand, the social philosopher Herbert Spencer, who was an evolutionist, saw society as being moulded by the survival of the fittest through struggle among individuals. In the twentieth century, DNA has replaced gross anatomical features as the supposed determinant of human nature. Now, we are told that the *genes* of blacks or of members of the working

class, rather than the shapes of their skulls, doom them to an inferior status in a species in which the status hierarchy is itself an inevitable result of the "human genotype."

The earlier forms of determinism have now been fairly well discredited. Thus, the contention of Jensen and Herrnstein that there is a high heritability for IQ and a genetic difference between races or between social classes has been thoroughly debunked. Much of the evidence for high heritability has turned out to be either "cooked" or grossly biased(1). (See the article by Schwartz in this volume.) Moreover, it has been pointed out repeatedly that the developmental and environmental plasticity of a character are not related to the presence or absence of genes that influence the character. Further, the demonstration that some trait differs genetically between *individuals* does not give any information about the causes of differences between *groups* such as races and social classes(2).

The simplistic forms of the human nature argument given by Lorenz, Ardrey, Fox and Tiger, and others have no scientific credit and have been scorned as works of "advocacy" by E. O. Wilson. His book, *Sociobiology: The New Synthesis*(3), is the manifesto of a new, more complex version of biological determinism. But Wilson's *Sociobiology* is no less a work of "advocacy" than its already rejected predecessors.

Wilson begins his book with a chapter entitled "The Morality of the Gene," in which he defines sociobiology as "the systematic study of the biological basis of all social behavior. For the present it focuses on animal societies. . . . But the discipline is also concerned with social behavior of early man and the adaptive features of organization in the more primitive contemporary human societies"(3; p. 4). The book as a whole is intended to "codify sociobiology into a branch of evolutionary biology," encompassing all human societies, ancient and modern, preliterate and postindustrial, since he believes that "sociology and the other social sciences, as well as the humanities, are the *last branches of biology* waiting to be included in the Modern Synthesis"(3; p. 4). [Our emphasis.] Like his predecessors, Wilson examines sociobiological data and theories in the light of ethical philosophy, with the objective of understanding the human situation with enlightened scientific thought.

This is not a mere academic exercise. For more than a century, the idea that human social behavior is determined by evolutionary imperatives and constrained by innate or inherited predispositions has been advanced as an ostensible justification for particular social policies. Deterministic theories have been seized upon and widely entertained not so much for their alleged correspondence to reality but for their more obvious political value, their value as a kind of social excuse for what exists. A few examples illustrate the point.

Eight years before Darwin published the *Origin of Species,* Herbert Spencer, a nineteenth-century apostle of determinism, argued in *Social Statics*(4) that it was unnatural to attempt the eradication of poverty by social welfare schemes since "the poverty of the incapable, the distresses that come upon the imprudent, the starvations of the idle . . . are the decrees of a large, far-seeing benevolence . . . under the natural order of things society is constantly excreting its unhealthy, imbecile, slow, vacillating, faithless members . . ."(4; p. 353).

As early as 1900, some leaders of industrial corporations understood that there was something to be gained from the biologizing of social institutions. In that year, the head of the immense armaments firm of Alfred Krupp, later one of the principal supporters of Hitler and a principal beneficiary of his slave-labor camps, offered a lucrative cash prize to the winner of an essay contest on the question, "What does the theory of descendence teach us in regard to the internal political development and legislation of states" (quoted in Zmarzlick [5; 457-58]). Somewhat later, John D. Rockefeller, an admirer of Spencer, told a Sunday school class that "the growth of a

larger business is merely survival of the fittest . . . the working out of a law of nature and a law of God" (quoted in Hofstadter [6; p. 45]).

Ethologists and other determinists have not always left it to others to draw political conclusions from their works. Consider the case of the Nobel Prize winning ethologist, Konrad Lorenz. Perhaps "those who have been charmed by his film of himself leading, like a mother goose, a brood of greylag geese about the farm will recoil from identifying his works as political"(7; p. 124). Yet, writing in Germany, in 1940, when the slave-labor program and other genocidal policies were already in effect, Lorenz explained that the traditional ethical philosophy of modern civilization had created biological problems comparable to those in the domestication of animals. In the process of civilization, we have lost certain "innate releasing mechanisms" that are normally retained to maintain purity of breeding stocks: "The selection for toughness, heroism, social utility . . . must be accomplished by some human institution if mankind, in default of selective factors, is not to be ruined by domestication-induced degeneracy. *The racial ideal as the basis of the state has already accomplished much in this respect*"(8; p. 71). "We must—and may safely—rely on the healthy intuition of the best elements among us and entrust to them the choices that will determine whether the nation flourishes or deteriorates. If this selection fails, *and the defective elements are not eliminated,* they will permeate our nation . . . just like the cells of a malignant growth . . ."(8; p. 75). [Our emphasis throughout.]

OUTLINE OF THE THEORY

In order to make their case, determinists construct a selective picture of human history, ethnography, and social relations. They misuse the basic concepts and facts of genetics, evolutionary theory, and ethology to assert things to be true that are unknown, ignoring whole aspects of the evolutionary process and stating that conclusions follow from premises when they do not. They then invent *ad hoc* hypotheses to take care of the contradictions and rely on a form of "scientific reasoning" that leads to conclusions that are untestable.

Like other deterministic theories, sociobiology begins by attempting to establish the existence of a syndrome of social and individual behaviors that is characteristic or universal in the human species. However, it is often unclear to what extent a particular trait is claimed to be universal. So, for example, in Wilson's *Sociobiology,* we are told that ". . . true spite is commonplace in human societies" (p. 119) and ". . . that man would rather believe than know" (p. 561), and that "human beings are absurdly easy to indoctrinate" (p. 562). These assertions leave unclear how much variation in behavior is implied by the words "common," "rather," and "absurdly easy." These are terms that arise from a world view in which individual human beings and societies are seen as manifesting, to different degrees, their essential "natures" and "tendencies." It is no accident that the description of this underlying essential nature bears a remarkable resemblance to the society inhabited by the theorists themselves. In Wilson's case, it is the modern market-industrial-entrepreneurial society of the United States.

Having established a picture of human nature that conforms to their immediate world, sociobiologists try to show that this has arisen in the course of human evolution through natural selection. To do so, they carry out a four-step theoretical argument. First, they try to establish that "human nature" and social organization are only special examples of more general categories of behavior and social organization in the animal kingdom. In this way, they try to establish an evolutionary history for these social phenomena. The biological continuity is sometimes asserted to be a direct

genetic continuity between humans and other primates through common ancestry (*evolutionary homology*) and sometimes to be the result of common forces of natural selection that operate on different genetic bases in more distantly related organisms and produce evolutionary convergence (*evolutionary analogy*). Both arguments depend, as we will see, on an anthropomorphic treatment of the behavior of other animals.

By necessity, the second step is the assertion that the different aspects of human social behavior are, to various degrees, genetically programmed. This step is essential because the modern theory of evolution by natural selection demands that the evolving trait have a genetic base. Without genetic variation, there can be no evolution in a *biological* sense (although there may be in a cultural sense). Sociobiologists must be equally opposed to the psychological determinism of Skinner and to any theories of human psychosocial freedom that involve an autonomous development of human history according to its own dynamic.

In fact, no evidence is offered by sociobiologists for a genetic basis of human social behavior beyond arbitrary postulations of genes and the obvious fact that people and apes are genetically different. The chief error of this part of the theory is the supposition that the arbitrary elements into which human behavior has been partitioned by natural and social scientists (elements like "xenophobia," "territoriality," "spite," "conformity," "religion," and so on) are based on underlying elements of heredity. It is the classic error of reification by which mental constructs are treated as if they represented concrete, objective realities.

Third, sociobiologists must argue that each element of "human nature" is in itself adaptive, that is, that it leads to higher reproductive fitness. For each characteristic, such as love, spite, entrepreneurship, xenophobia, and so on, they must invent a plausible story to explain why possessors of the trait might leave more offspring in the remote future.[1] It is not necessary to show that the individual will leave more immediate progeny, provided one can show that the individual's relatives are sufficiently benefited (*kin selection*). Alternatively, an "altruistic" act toward an unrelated person might elicit a future response of benefit to the actor. As we will show, such wide latitude is left for imaginative invention that no conceivable observation could be at variance with this theory. A little ingenuity will always lead to an adaptive explanation. A theory with this much "give" is worthless, because it is nonfalsifiable.

Finally, sociobiology must cope with the fact that human societies differ very much from each other in their cultural form and content and that, despite the predictions and observations of population genetics of the slow rates of *genetic* change and the small *genetic* differentiation among human populations, many societies have changed very rapidly. However, the possibility of using population genetics as a quantitative test of the theory is closed off by the *ad hoc* introduction of a "multiplier effect"(3; pp. 11-13) that allows sociobiologists to equate any observed amount of cultural differences arbitrarily with any postulated degree of genetic change. What follows is a detailed examination of each of these elements of sociobiological theory, especially as elaborated in E. O. Wilson's *Sociobiology*.

Like other works of scientific advocacy, *Sociobiology* cannot be read as a consistent or rigorous exposition. There are numerous direct contradictions on important points, and sometimes specific assertions contradict the general arguments of the work as a whole. For example, in one place Wilson says that "genes have given away most of their sovereignity"(p. 550) and, in another, that "it does not matter

[1]Strictly speaking, evolutionary theory requires that the *genes* of those possessing the trait must be more represented in future generations.

whether aggression is wholly innate or acquired partly or wholly by learning" (p. 255). But, if taken at face value, these statements are completely at variance with the central point of *Sociobiology:* that natural selection has established genes in the human species that are responsible for the human social condition. Such contradictions enable the author, when criticized, to claim to have disproved the critics by choosing the appropriate statements. This is another example of the "plasticity" that has been built into Wilson's arguments.

A VERSION OF HUMAN NATURE

Since *Sociobiology* is an attempt to explain the detailed structure of human societies as the result of biological evolutionary adaptation through natural selection, its first task is to delineate the model of human nature that it needs to explain. While the model is sometimes explicitly stated (for example, *territoriality* and *tribalism* are explicitly stated [pp. 564-65] to be human universals), more often its elements are embedded in more general discussion.

Among Wilson's various aspects of human nature are:

1. Indoctrinability—"Human beings are absurdly easy to indoctrinate—they seek it"(p. 562).

2. Spite and family chauvinism—"True spite is commonplace in human societies, undoubtedly because human beings are keenly aware of their own blood lines and have the intelligence to plot intrigue" (p. 119). (Undoubtedly? See Harris [9; pp. 369-91].)

3. The nuclear family, with male dominance and superiority—"The building block of nearly all human societies is the nuclear family. . . . Sexual bonds are carefully contracted . . . and are intended to be permanent . . ." (p. 553). Among "general social traits in human beings" are listed "aggressive dominance systems, with males dominant over females" (p. 552).

4. Reciprocal altruism (as opposed to true unselfishness)—"Human behavior abounds with reciprocal altruism," as, for example, "aggressively moralistic behavior," "self-righteousness, gratitude, and sympathy" (p. 120).

5. Blind faith—"Men would rather believe than know" (p. 120).

6. Genocide (p. 575) and warfare (p. 572)—"The most distinctive human qualities" emerged during the "autocatalytic phase of social evolution" that occurred through "intertribal warfare" and through "genocide" and "genosorption." While social evolution may no longer occur in this fashion, we are prisoners of our own evolutionary history. "Some of the functions are almost certainly obsolete, being directed toward such Pleistocene exigencies as hunting and gathering and intertribal warfare" (p. 575).

7. Xenophobia (fear of strangers)—"Part of man's problem is that his intergroup responses are still crude and primitive, and inadequate for the extended territorial relationship that civilization has thrust upon him. . . . Xenophobia becomes a political virtue" (p. 656).

This list is not exhaustive and is meant only to show how myopically "human nature" is viewed, as though its essentials were manifested in modern Euro-American culture as described by orthodox social science.

To construct such a view of human nature, Wilson either must divorce himself from any historical or ethnographic perspective or engage in bad ethnographic and bad historical reporting. His discussion of the economy of scarcity is an excellent example. An economy of scarcity and unequal distribution of rewards is described as an aspect of human nature, as follows: "The members of human societies sometimes cooperate

closely in *insectan* fashion, but more frequently they compete for the limited resources allocated to their role sector. The best and most entrepreneurial of the role-actors usually gain a disproportionate share of the rewards, while the least successful are displaced to other, less desirable positions" (p. 554). [Our emphasis.]

Note that this description of modern market-economy society is universalized over time and space, consigning the unstated cooperative "exceptions" to an "insectan" form of behavior. It is rather reminiscent of the predetente rhetoric about "insectlike" Chinese millions, identically blue clad and mindlessly regimented into cooperative (and "unnatural") brigades. In fact, there exists a great deal of ethnographic description that completely contradicts this conception of social organization. There are scores of known "egalitarian" societies that do not have more or less desirable positions or inequalities of shares. (See, for example, the literature on Eskimos [9, 10, and 11].) Wilson's description of the economy of scarcity as universal ignores the following types of present and past societies.

1. Those *not* differentiated in any significant way by "role sectors."
2. Those *without* scarcities that are differentially induced by institutions for different subpopulations of the society (i.e., all members have equal access to all resources and, in many documented cases, have lived, apparently indefinitely into the past, *without* having approached the limits of their resources).
3. Those *not* differentiated by lower and higher ranks or strata (see Krader [12; pp. 29-42] or chapters 2 and 3 of Fried [13], which contrast egalitarian societies with rank and stratified societies).
4. Those *not* characterized by depressed or deprived access to societal rewards.

Most of these "egalitarian" societies enjoy a state of universal access to resources that has been labeled, by some, "affluent"(11). It has been well known for at least 100 years(14) that, in the "simpler" societies of the Old and New Worlds, people shared food and held possessions in common and worked only a few hours daily to meet their needs. The evidence from such societies is particularly devastating because it is upon these very groups that Wilson tries to build his hypothetical reconstruction of ancestral societies.

Wilson's conceptualization of gender roles[2] and male "dominance" serve as other examples of his attempt to create a human nature. *Sociobiology* conveys a powerful underlying statement that substantiates male supremacy in humans and other animals. The sexist message in the book is carried in subtle and implicit ways.

First, Wilson and other sociobiologists perpetuate the common sexist usages of the English language. In *Sociobiology*, animal behavior is sometimes described in anthropomorphic terms that have strong sexist connotations. Two examples should here suffice: it is stated that courtship display (usually males toward females) "can be envisioned as a contest between *salesmanship* and *sales resistance*" and that there "will be a strong tendency for the courted sex to develop *coyness*"(p. 320). [Our emphasis.] Later, on the same page, we are told that "rampant *machismo* has evolved in some insects. . . ." Chapters 13 through 16, in particular, are replete with additional examples of this use of language. "Man" and "mankind" are frequently employed when Wilson is referring to humans, and his use of "he" and "his" as the universal pronouns further demonstrates the sexism throughout the book.

Second, Wilson follows the culturally pervasive tendency to emphasize or place higher value upon what is male. In *Sociobiology*, this is accomplished by describing female behavior after male behavior is described and by using more males in

[2]Oakley(15), Tresemer(16), and others have pointed out that it is more appropriate to use the term "gender" when speaking of social behavior and to use the word "sex" for biological characteristics.

illustrations. Such a subtle but effective organization reveals the male supremacist perspective of the book. In photographs and illustrations where only one sex is shown, it is rarely a female; where both sexes are recognizable, usually the females are peripheral in placement and activity—the male is outstanding. Explicit captions leave no doubt of the secondary status of the female.

Third, the sweeping generalizations about human behavior, which are pivotal to Wilson's conclusions, have been made with few references to the work of anthropologists, ethnographers, and sociologists. Such work is often full of sexist assumptions, since systematic bias occurs when, for example, the ethnographer describes women by using information presented by the men in a society or assumes that the men's view of the value of women's activities is shared by all members of the society. Wilson draws upon studies that suffer from such sexist bias without criticizing them. Another striking point is the paucity of ethnographic data he utilized. To bolster his analysis of male dominance, Wilson cites Fox(17), so that Wilson's "evidence" is an interpretation of Fox's book, which itself is an interpretation of selected ethnographic reports. Interestingly, Tiger and Fox(18) were dismissed (p. 551) for being "inefficient and misleading" in their analysis of human social behavior and for using the "advocacy method" in reconstructing human social evolution (p. 28).

Indeed, it must be recognized that sexism, the *socially prescribed* power men have over women, does exist in most societies. Such a cultural and political arrangement, which varies from one society to the next(15, 19, and 20), is quite different from the "dominance hierarchy" that Wilson applies to many animal groups.

Wilson's view of aggression and warfare provides yet another example of his efforts to ascribe a general biological basis to social behaviors that prevail in Western societies. The historical and ethnographical data together indicate a steady increase in absolute and relative rates of killing in warfare as one moves toward large-scale, complex, and increasingly "modern" societies. "Primitive" warfare is rarely lethal to more than one, or at most a few, individuals in an episode of warfare and is virtually without significance genetically or demographically(21). Warfare in "simple" societies is often carried out in skirmishes and single combat. A wounding may be enough to call off the "war." Genocide was practically unknown until state-organized societies appeared in history (as far as can be made out from the archaeological and documentary records). Cave art of the Pleistocene seldom shows killing of persons with weapons, only hunting of animals(22; pp. 74-76; 23; pp. 80-82).

Moreover, the use of the present state of "primitive" people to reconstruct human history is highly suspect among anthropologists. Virtually all known primitives have lived in the coercive environments of state-organized societies characterized by warfare, imperial expansion, and imposition of exclusive ethnic definitions or tribute and taxes on external groups, the so-called tribes(13; pp. 170-74). Today, many anthropologists think that the characteristics of all presently known tribal peoples may be a function of the structural relationship to state-organized societies. Not a primordial "primitive" condition at all, "tribalism" is generated by the presence of the state, which has replaced prior household and cooperative locality groupings.

It is in the description of human nature that sociobiologists must invoke what is the first in a series of ploys designed to make the system of explanation proof against any possible test. Realizing that history and ethnography do not support the universality of his description of human nature, Wilson claims that the exceptions are "temporary" aberrations or deviations. Thus, although genocidal warfare is (assertedly) universal, "It is to be expected that some isolated cultures will escape the process for generations at a time, in effect reverting *temporarily* to what ethnographers classify as a pacific state" (p. 574). [Our emphasis.]

This is nonsense on several grounds. First, as was indicated earlier, most known "primitive" societies do not commit genocide in any meaningful contemporary usage of that term. Second, most ethnographers do not use classifications like "pacific states." Third, if a group, known to have committed genocide once, does not do so for generations, it is absurd to assert that the society is really essentially genocidal but is temporarily not manifesting its real nature.

Another related ploy is the claim that ethnographers and historians have been too narrow in their definitions and have not realized that apparently contradictory evidence is really confirmatory. Thus, "Anthropologists often discount territorial behavior as a general human attribute. This happens when the narrowest concept of the phenomenon is borrowed from zoology . . . it is necessary to define territory more broadly . . . animals respond to their neighbors in a highly variable manner. . . . The scale may run from open hostility . . . to oblique forms of advertisement or *no territorial behavior at all.*" [Our emphasis.] "If these qualifications are accepted, it is reasonable to conclude that territoriality is a general trait of hunter-gatherer societies" (pp. 564-65).

These are examples of a general method employed by sociobiologists in an advocacy procedure that guarantees that no observation can contradict the theory. First, they make definitions that *exclude nothing,* for example, territoriality. If a particular behavior appears to be absent, it is claimed that it is present in a different form. This technique has much in common with Freud's theory of "defense mechanisms," according to which a psychic drive may appear in a disguised form, even reversed (apparent love is really hate) or not at all, because it is "repressed." Second, Western conceptual categories are reified and given an objective and concrete reality that they do not have. Anyone who tries to give concepts like "religion," "entrepreneurship," and "warfare" precise technical meanings soon discovers that they are arbitrary and that the boundaries they reify do not exist in nature. The Martian zoologist invented in Wilson's Chapter 27 would be most unlikely to use any of the biased conceptual categories on which the argument of *Sociobiology* is built.

HUMANS AS ANIMALS—THE MEANING OF SIMILARITY

To support a biological explanation of human institutions, it is useful, but not necessary, for sociobiologists to claim an evolutionary relationship between the nature of human social institutions and "social" behavior in other animals. First, such a relationship would be indirect evidence that human social behavior has a genetic basis, and, second, it would make more plausible the argument that human social behavior has evolved under the influence of natural selection.

Evolutionary biologists make a distinction between two kinds of similarity: *homology* and *analogy*. *Homologous* features are retained from common ancestors by way of genes inherited from the common ancestors—the bones of a human arm and a bat's wing, for example. Obviously, sociobiologists would prefer to claim homology as the basis for similarity in behavior between humans and other animals, for then they would have a *prima facie* case for genetic determination. In some sections of *Sociobiology* (table 27-1, for example), Wilson attempts to do this by listing "universal" features of behavior in higher primates, including humans. But claimed external similarity between humans and our closest relatives (which are by no means extremely close to us) does not imply identity of cause. Reciprocal altruism may exist in higher primates and, for all we know, it may be genetically coded in them. But the same behavior in humans may be purely learned and widely spread among human cultures for the simplest of all reasons: successful and complex social structures

demand cooperation for mutual benefit, and the enormous flexibility of the human brain permits this adaptive action as one facet of its vast range of potential behaviors.

Analogous features share similar forms and functions, but represent separate convergent evolutionary developments mediated by similar forces of natural selection, the wings of a bat and an insect, for example. More often than not, Wilson argues from analogy. Such arguments operate on shaky grounds. They can never be used to assert genetic similarity, but they can be used as a plausibility argument for natural selection of human behavior by the following logic: first, humans and some other animals perform a similar but only analogous behavior; second, since this behavior is adaptive, it would be favored by natural selection; and, third, natural selection has operated on different genes in the two species, but it produced the same response as an independent adaptation to similar environments.

The argument is not even worth considering unless the similarity is so precise that identical function cannot be reasonably denied, as in the classic case of evolutionary convergence, the eyes of vertebrates and octopuses. Here Wilson fails badly, for his favorite analogies arise by a twisted process of imposing human institutions upon animals by forced comparisons. Then, having imposed human traits upon animals by metaphor, he rederives the human institution as a special case of the more general phenomenon "discovered" in nature. In this way, human institutions suddenly become "natural" and are simply inheritances from an evolutionary past. (See Bookchin's article in this volume.)

A classic example, long antedating *Sociobiology,* is "slavery" in ants. "Slave-making" species capture the immature stages of "slave" species and bring them back to their own nests. When the captured workers hatch, they perform housekeeping tasks with no compulsion as though they were members of the captor species. Why is this "slave-making" instead of "domestication"? Human slavery involves, first, members of one's own species, second, continued compulsion, and, third, the slave as an article of exchange, a commodity engaged in the production of commodities. Slavery is a human economic institution in societies that produce an economic surplus, with the slave as a form of capital investment. It has nothing to do with ants, except by weak and meaningless analogy. Why, for that matter, do we speak of "queens" and "workers" with all the implications about the inevitability of social hierarchy? The so-called "queen" may be more a captive of the "workers" than their ruler. In many species, she is nothing more than a laying machine, bloated with eggs and forced by the "workers" to remain in one spot and to produce continually.

Wilson goes beyond ant hierarchies by expanding the realm of these mock analogies to find rudimentary forms of barter (p. 551), division of labor (p. 298), culture (p. 559), ritual (p. 560), and esthetics (p. 564) among animals. But, if we insist upon seeing animals in the mirror of our own social arrangements, we cannot fail to find any human institution we want among them. This is argument by analogy in its most inadmissible sense.

GENETIC BASES OF BEHAVIOR

We can dispense with the direct evidence for a genetic basis of various human social forms in a single word: *none.* The genetics of human social behavior is in a rudimentary state for both methodological and political reasons. Because we cannot reproduce particular human genotypes over and over or experimentally manipulate the environments of individuals or groups, it has been impossible to gather even the rudiments of information about how genes influence human behavior. That is the methodological problem. Because certain biological determinists have a strong desire to

convince people that intelligence, criminality, and violence are mostly the fault of genetic differences, a great deal of bad data, incorrect analyses, and even outright misrepresentations have been propagated in recent years, obscuring what was already a muddy prospect. That is the political problem. There is no evidence that conforms to the elementary rules of experimental design for the existence of genetic differences between individuals or groups with respect to social phenomena. Nor is there any evidence that such traits as xenophobia, religion, ethics, social dominance, hierarchy formation, slave-making, and so on, are in any way specifically coded in the genes of human beings. And, indeed, Wilson offers no such evidence. As a result, he makes a series of confused and contradictory statements about what is an essential element in his argument. So, for example, he speaks of "genetically programmed sexual and parent-offspring conflict" (p. 563), yet there is the "considerable technical problem of distinguishing behavioral elements and combinations that emerge . . . independently of learning and those that are shaped at least to some extent by learning. . . . Where both processes contribute, their relative importance under natural conditions is extraordinarily difficult to estimate" (p. 159). That is putting the difficulty mildly. Elsewhere, the capacity to learn is stated to be genetic in the species, so that "it also does not matter whether aggression is wholly innate or acquired partly or wholly by learning" (p. 255). But it does matter. If all that is genetically programmed into humans is "that genes promoting flexibility in social behavior are strongly selected" (p. 549) and if "genes have given way most of their sovereignty" (p. 550), then biology and evolution can offer no insight except the most trivial one that the *possibility* of social behavior has evolved. But Wilson does not believe that genes have given up their sovereignty where it counts, since, in the next phrase, he states that genes "maintain a certain amount of influence in at least the behavioral qualities that underlie the variations between cultures." He has stated as a *fact* that genetic differences underlie variation between cultures, when no evidence at all exists for this assertion and when some considerable evidence is against it. The only citation of evidence is that "moderately high heritability has been documented in introversion-extroversion measures, personal tempo, psychomotor and sports activities, neuroticism, dominance, depression and the tendency toward certain forms of mental illness, such as schizophrenia" (p. 550). But, except for recent studies of schizophrenia(24), there is no evidence for the inheritance of any of these traits, nor is there any evidence of any kind for genetic differences between cultural groups in these traits. Indeed, we are not told why sports activities, neuroticism, and schizophrenia are "behavioral qualities that underlie the variations between cultures."

If we take Wilson at his word, that he believes genes to have some role, but not an exclusive one, in cultural variation, then there is a large gap in the theoretical structure. For, if genes are not everything, then the dynamics of the evolution of human culture must include not only the rules of Mendelian inheritance but some theoretical structure of cultural transmission as well. Yet, in a work that claims to lay the foundations of a theory of the evolution of society, there is no hint of a theory of cultural transmission or of the possible interaction of cultural and genetic transmission. It is not that there is no completely articulated theory of cultural transmission, it is that there is no consideration of the problem at all. On the contrary, the problem of cultural transmission is dismissed by characterizing Dobzhansky's position, "Culture is not inherited through the genes, it is acquired by learning from other human beings," as an "extreme orthodox environmentalism" (p. 550). But Wilson's *effective* position, all qualifying phrases to the contrary notwithstanding, is an extreme hereditarian one, since the only dynamic of transmission and evolution considered is natural selection operating on Mendelian genes.

Since Wilson can adduce no facts to support the genetic basis for human social behavior, he tries two tacks. First, he uses the suggestion of evolutionary homology between primates and humans to imply a genetic basis in our species. But, as we discussed earlier, the evidence for homology as opposed to analogy is very weak. More important, the phylogenetic method does not tell us whether the traits that are relatively constant from group to group, the so-called *conservative traits,* are those that are most likely to be under direct genetic control or whether it is the traits that vary from group to group, the *labile traits,* that are genetically determined. In fact, Wilson confuses the issue by claiming both. At first, he claims that conservative traits are most probably homologous, but then he hedges: "The possibility remains that some labile traits are homologous between man and, say, chimpanzee. And conversely, some traits, conservative throughout the rest of the primates, might nevertheless have changed during the origin of man" (p. 551). Then, he reverses the argument to "heuristically conjecture" that phenotypically *labile* traits are those that are most likely to differ from one human society to another on the basis of genetic differences. The phylogenetic method is thus reduced to uselessness, since both constancy and lability are to be taken as evidence for a genetic basis. We are not told by the sociobiologist what sorts of traits are *not* likely to be genetically based. We must agree with Wilson that "the comparative ethological approach does not in any way predict man's unique traits. It is a general rule of evolutionary studies that the direction of quantum jumps is not easily read by phylogenetic extrapolation" (p. 551).

The second main tack is to postulate genes right and left. The technique is a simple one. Start by saying "if there were a gene for spite" and then go on with a long discussion of the consequences, dropping the "if" along the way. There are hypothetical altruist genes, conformer genes, spite genes, learning genes, homosexuality genes, and so on. An instance of the technique is on pp. 554-55. "Dahlberg (1947) showed that *if* a single gene appears that is responsible for success and upward-shift in status. . . . Furthermore, there are *many* Dahlberg genes. . . ." [Our emphasis.] Or, again (p. 555), "The homosexual state itself results in inferior genetic fitness. . . . It remains to be said that if such genes really exist they are almost certainly incomplete in penetrance and expressivity." The notion of confirming the existence of behavior genes that only have effect in some unspecified proportion of individuals carrying them (*incomplete penetrance*) and with some unspecified range of manifestation when they do have effect (*variable expressivity*) is interesting. (A standing joke among human geneticists is that, when some trait has defied attempts to unravel its inheritance [if any], it is said to be "a dominant gene with incomplete penetrance and variable expressivity.")

Another example is on p. 562: "If *we assume* for argument that indoctrinability evolves . . ." and "societies containing higher frequencies of conformer genes replace those that disappear. . . ." [Our emphasis.] Or how about "innate moral pluralism" (pp. 563-64)? For other examples, see nearly any page of Trivers (25 or 26).

The vagueness and contradictions of the genetic arguments appear at every level. Are we to assume that the human species is genetically uniform for the genes producing "human nature"? If so, how are we ever to detect such genes? On the other hand, perhaps we are genetically variable. If so, what mechanisms maintain the variation? Despite an early chapter on the mathematics of population genetics, not a single result of this highly developed theory, which addresses the issue of how genetic variation is maintained, is applied by Wilson to his speculations on behavioral genes because of the vagueness of his genetic hypotheses. The mathematical theory is so much window dressing.

Finally, we do return again to the error of reification. Geneticists long ago abandoned the naive notion that there are genes for toes, genes for ankles, genes for

the lower leg, genes for the kneecap, and so on. Yet, the sociobiologists break the totality of human social behavior into arbitrary units, call these elements "organs" of behavior, and postulate particular genes or gene complexes for each.

EVERYTHING IS ADAPTIVE

The next step in the sociobiological argument is to try to show that the hypothetical genetically programmed behavior organs have evolved by natural selection. Two assertions are made, the first explicit, and second implicit: first, since natural selection insures that any genes that confer higher reproductive success on an individual or its relatives will increase in number in succeeding generations, behaviors and social structures that we observe must exist because they are *or were* adaptive; second, a trait that increases an individual's status, power, and wealth also increases its reproductive success.

There is relatively little discussion of this second assertion by sociobiologists, although, if it is not true, their argument falls. If natural selection is to have established the behavioral characters that lead to status and wealth, it can do so *only* if those characteristics somehow lead to a higher reproductive success. Evidence referred to by Wilson (p. 288) states that among laboratory mice, *Anubis* baboons, and Yanamamo Indians, the "dominant" males impregnate a disproportionate share of females. In fact, in the ethnographic literature, there are numerous examples of groups whose political "leaders" do not have greater access to mates (e.g., the Yaruro or American society). In general, it is difficult to demonstrate a correlation of any of the sociobiologists' "adaptive" social behaviors to actual differential reproduction.

The first assertion, that all human behavior is *or has been* adaptive, is an outdated expression of Darwinian evolutionary theory, characteristic of Darwin's nineteenth-century defenders who felt it necessary to prove everything adaptive. Although modern evolutionary theory is cleansing itself of such panselectionism, sociobiologists maintain the old line. It is a deeply conservative politics, not an understanding of modern evolutionary theory, that leads one to see the wonderful operation of adaptation in every feature of the living world. There is no hint in Wilson that evolutionary geneticists are deeply divided on the question of how important adaptive processes are in manifest evolution. Most evolutionary geneticists will admit that some fraction (the argument is over how large a fraction) of evolutionary change has occurred either in the absence of natural selection and adaptation or as an interaction between adaptive and random processes.

Second, and more important, modern evolutionary theory gives a large place to the *indirect* effects of selection. That is, organs or traits that are not themselves under direct natural selection may change their size or shape as a consequence of developmental or physiological links to features that are under direct selection. So, for example, many populations of animals undergo severe oscillations in numbers, not because natural selection has favored oscillation, but because selection has favored concentrating high reproduction in one age class, and that, *incidentally and for purely dynamical reasons*, leads to population oscillation. Indirect selection is a strain in evolutionary thought that goes back to Julian Huxley's(27) concept of *allometry*, by which he sought to avoid much of the tortured logic required by naive theories of direct selection(28). Allometry is the differential rate of growth of organs so that larger animals do not have all of their organs proportionately larger. For example, among species of primates, teeth do not increase proportionately with increased body size. Thus, large primates have proportionately smaller teeth than small primates as a result of a purely developmental relationship. Many evolutionary trends have been explained

in this way, and it seems at least reasonable that many features of human social organization, if not all, may be involved in a complex system of correlation with other changes (for example, increased plasticity of neurological response and cognitive capacity.)

Thus, the major assertion of sociobiologists that behaviors and social structures exist because of their superior adaptive values is only an assumption that is nowhere tested. In fact, no tests are even proposed or any criteria established to allow a distinction between the natural selection model, historical models, and religious beliefs, as explanations for the diversity of manifest behavior. We cannot distinguish the possibility that parent-offspring conflict is the result of natural selection for conflict genes (p. 341) from the possibility that parent-offspring conflict in humans is an historically conditioned outcome of certain forms of social and economic arrangement. It never occurs to Wilson to suggest a test of the alternative hypothesis that "conformity" has not been selected by differential reproduction (pp. 561-62); instead, he claims that conformism and indoctrination result from an application of the ideology expressed by Daniel Webster when he said, "Education is a wise and liberal policy, by which property and life and the peace of society may be secured."

When we examine carefully the manner in which sociobiology pretends to explain all behaviors as adaptive, it becomes obvious that the theory is so constructed that *no tests are possible.* There exists no imaginable situation that cannot be explained; it is *necessarily confirmed by every observation.* The mode of explanation involves three possible levels of the operation of natural selection: one, classical individual selection to account for obviously self-serving behaviors; two, kin selection to account for altruistic or submissive acts toward relatives; and, three, reciprocal altruism to account for altruistic behaviors directed toward unrelated persons. All that remains is to make up a "just-so" story of adaptation with the appropriate form of selection acting. For some characteristics, it is easy to invent a story. The "genes" for dominance, aggression, genocide, deception, and hypocrisy will "obviously" be advantageous at the individual level. For other behaviors, rather more ingenuity is required. For example, in a recent high school text produced by sociobiologists I. DeVore, G. Goethals, and R. Trivers(29), students are asked to give an evolutionary explanation of why children hate spinach, when adults like it. The answer: spinach contains a substance that makes calcium unavailable to the body; children need calcium for growing bones; adults no longer need much calcium and so can benefit from the other nutritional advantages of spinach; therefore, genes causing children to dislike spinach, but allowing adults to like it, will be favored by natural selection(29; p. 115).

A rather less trivial example is the attempt of sociobiologists to explain homosexuality (which they assume to be genetic without evidence). Homosexuality would seem to be at a reproductive disadvantage since "of course, homosexual men marry much less frequently and have far fewer children." (No account is taken of the high frequency of homosexual encounters among "heterosexual" men, as shown by Kinsey. Indeed, the whole typology of "homosexual" and "heterosexual" is in question. And what about "homosexual" women?) But a little imagination solves the problem: "The homosexual members of primitive societies may have functioned as helpers . . . [operating] with special efficiency in assisting close relatives" (p. 555). The genes would then be "sustained . . . by kin selection alone." Here we can see the postulate of kin selection, introduced by Hamilton (30), operating to save the day when direct selection fails. If you cannot think of a reason why a trait will confer direct fitness on its bearer, you need only imagine why it might help the *relatives* of the bearer. In terms of spreading one's genes, it is worthwhile to sacrifice your own life to save two sisters, eight first cousins, and so on, since you will then be propagating your genes indirectly.

Altruistic behaviors when directed toward nonrelatives can also be explained in a similar manner. Only one more imaginative mechanism is needed to rationalize such phenomena as friendship, morality, patriotism, and submissiveness even when the bonds do not involve relatives. The theory of reciprocal altruism(25) proposes that selection has operated so that risk taking and acts of kindness can be recognized and reciprocated and the net fitness of both participants is increased. It requires that the actor and the beneficiary be able to remember the act and calculate the costs and benefits of reciprocity.

The trouble with all these explanations is that they render the whole system unfalsifiable. Nothing is explained because *everything* is explained. When individuals are selfish, that is explained by simple individual selection. When, on the contrary, they are altruistic, it is kin selection or reciprocal altruism. When sexual identities are unambiguous, it is because individual fertility is increased. When, however, homosexuality is common, it is the result of kin selection. The sociobiologists give us no example that might conceivably contradict their scheme of perfect adaptation. If one should arise, we will be told by Wilson that it is a "temporary reversion," as in those tribes who are in "what demographers describe as a pacific state" (p. 574).

There do exist data supporting the idea that kin selection is effective for some traits in social insects, just as there are large quantities of data that show the existence of individual selection for many traits in many organisms. It is not the existence of kin selection nor individual selection as phenomena that is at issue. It is, rather, the arbitrary invocation of these effects, together with the untested story about "reciprocal altruism," that makes the total system of explanation for all human behavior inaccessible to any test or possibility of falsification.

One of the major consequences of the creation of an infinitely flexible and vague system of explanation is that a general atmosphere of easy but confused rationalization for any observation is created. Kin selection, reciprocal altruism, and the superior survivorship of whole groups (group selection) become mixed up in an attractive and vaguely plausible scheme to explain anything. Thus, E. F. Foulks explains the existence of schizophrenia(31): prophets and shamans (Joan of Arc and the Seneca prophet Handsome Lake are given as examples) frequently hallucinate, hear voices, interfere in others' thoughts, and are presumed to be schizophrenics. These people help their societies cope with unusual stresses by inducing or leading them to new social and political institutions, and the society in turn rewards them with high status. In this way, schizophrenics may have evolved to help "effect social change when traditional methods fail." (Joan's high status seems not to have been correlated with high fecundity.)

This sort of fantasy is likely to become a popular intellectual pastime in the near future. We may even envision a parlor game, "Find the Adaptation."

VARIATIONS OF CULTURE IN TIME AND SPACE

We have pointed out that adaptive evolutionary arguments can be rendered totally untestable by postulating genes wherever the theory requires them and by telling "just-so" stories about adaptation. There does exist, however, one possibility of tests of such hypotheses, where they make specific *quantitative* predictions about rates of change of characters in time and about the degree of differentiation between populations of a species. Population genetics, the analytical base on which evolutionary explanations are built, makes specific predictions about such rates, based upon estimates of mutation rates, population sizes, selection intensities, and migration patterns. In addition, there are hard data on the degree of genetic differentiation

between human populations for biochemical traits(32). Both the allowable rates of *genetic* change in time, from theory, and the observed *genetic* differentiation between populations are too small to agree with the very rapid changes that have occurred in human *cultures* historically and the very large *cultural* differences observed among contemporaneous populations.

To take a particular case, we can consider the rise and fall of Islam. In the sixth century A.D., the Arab world was made up of poor, pastoral people. In A.D. 622, Mohammed appeared, and, within 200 years, Islam was the supreme power in the West, both politically and culturally. Music, poetry, mathematics, science, historiography, all flowed in profusion from Baghdad and Cordoba, the two great capitals of power and culture in the tenth century. But, by 1492, the last Moors had been ejected from Spain and the Arab people were again a poor pastoral culture. This cycle, occurring over fewer than 30 generations, was too rapid by orders of magnitude for any large change by natural selection. It would appear that really significant cultural changes occur by some nongenetic means and that an understanding of human history and fortunes must be sought where it has always been, not in genes but in culture.

The same problem arises from the immense cultural differences between contemporaneous groups, since we know from studies of enzyme-specifying genes that at least 85 percent of that kind of human variation lies *within* any local population or nation, with a maximum of about 8 percent between nations and 7 percent between major races(33).

Wilson acknowledges and deals with both of these dilemmas with one bold stroke—he invents a new phenomenon. It is called the "multiplier effect" (pp. 11-13 and pp. 569-72). The multiplier effect postulates that very small differences in the frequencies of genotypes will result in major cultural differences. Thus, only a small increase in the frequency of "superior" types among the Arabs might result in immense changes in their political power and cultural riches. In like manner, very small differences in the frequency of hypothetical genes for altruism, conformity, indoctrinability, role playing, and so on, could move a whole society from one cultural pattern to another, even though most of the people were not different biologically. In a discussion of the multiplier effect (pp. 11-13), Wilson contrasts the large differences in behavior between closely related species of insects and of baboons, with supposed small differences in genes between the species. There is, however, no evidence about the amount of genetic difference between these closely related species, nor do we know how many tens or hundreds of thousands of generations separate the members of these species pairs since their divergence. Thus, we cannot say whether the behavioral differences are large or small relative to the genetic divergence or to the time of separation of these organisms.

There is the added problem of why nonhuman animals do not show equally rapid evolution and equally dramatic interpopulation variation in "social" traits. This anomaly is disposed of by yet another *ad hoc* hypothesis, the "threshold effect" (p. 573), according to which organisms must reach a certain (unspecified) level of social complexity before the multiplier effect will operate. Thus, some of the "'noblest' traits of mankind including team play, altruism, patriotism, bravery on the field of battle" may be the "genetic product of warfare," but the threshold effect prevents their development in lower animals (p. 573). But the multiplier effect, by which any arbitrary but unknown genetic difference can be converted into any cultural difference you please, and the threshold effect, by which any arbitrary level of social development can be postulated to account for any failure of generality, are pure inventions of convenience without any evidence to support them. They have been created out of whole cloth to seal off the last loophole through which the theory might have been tested against the real world.

AN ALTERNATIVE VIEW

It is often stated by biological determinists that those who oppose them are "environmental determinists" in the manner of Skinnerian psychology who believe that the behavior of individuals is precisely determined by some sequence of environmental events in childhood and youth. But such an assertion misses the point and reveals the essential narrowness of viewpoint in such deterministic ideologies. First, these ideologies see the *individual* as the basic element of behavior and determination, while society is simply the sum of all the individuals in it. For biological determinists, social institutions result from natural selection operating on individuals either directly or through their kin. But the truth is that the individual's social activity is to be understood only by first understanding social institutions. We cannot understand what it is to be a slave, or a slave owner, without first understanding the *institution* of slavery, an institution that defines both slave and owner.

Second, determinists assert that the evolution of societies is the result of changes in the frequencies of different sorts of individuals within them. But this confuses cause and effect. Societies evolve because social and economic activity alter the physical and social conditions in which these activities occur. Unique historical events, actions of some individuals, and the alteration of consciousness of masses of people interact with social and economic forces to influence the timing, form, and even the possibility of particular changes, but the individuals are not totally autonomous units whose individual qualities determine the direction of social evolution. Feudal society did not pass away because some autonomous force increased the frequency of entrepreneurs. On the contrary, the economic activity of Western feudal society itself resulted in a change in economic relations that made serfs into peasants and then into landless industrial workers, with all the immense changes in social institutions that were the result.

Finally, determinists assert that the possibility of change in social institutions is limited by the biological constraints on individuals. But we know of no relevent constraints placed on social processes by human biology. There is no evidence from ethnography, archaeology, or history that would enable us to circumscribe the limits of possible human social organization. What history and ethnography do provide us are the materials for building a theory that will itself be an instrument of social change.

References

1. Kamin, L. 1974. *The science and politics of I.Q.* New York: Halsted Press.
2. Lewontin, R. C. 1970. Race and intelligence. *Bulletin of Atomic Scientists* 26:2-8.
3. Wilson, E. O. 1975. *Sociobiology: The new synthesis.* Cambridge, Mass.: Harvard University Press.
4. Spencer, H. 1851. *Social statics: The conditions essential to human happiness specified, and the first of them developed.* London: Chapman.
5. Zmarzlick, H. G. 1973. Social Darwinism in Germany viewed as an historical problem. In *From republic to reich: The making of the Nazi revolution*, ed. H. Holborn. New York: Vintage Books.
6. Hofstadter, R. 1955. *Social Darwinism in American thought.* Boston: Beacon Press.
7. Eisenberg, L. 1972. The human nature of human nature. *Science* 176:123-28.
8. Lorenz, L. 1940. Durch Domestikation verursachte Störungen arteigenen Verhaltens. *Zeitschrift für angewandte Psychologie under Characterkunde* 59:56-75. [Quoted in Cloud, W. 1973. Winners and sinners. *The Sciences* 13:16-21.]
9. Harris, M. 1968. *Culture, man and nature.* New York: Cromwell.
10. Birket-Smith, K. 1959. *The eskimos.* 2nd ed. London: Methuen.
11. Sahlins, H. 1972. *Stone age economics.* Chicago: Aldine-Atherton.
12. Krader, L. 1968. *Formation of the state.* Englewood Cliffs, N.J.: Prentice-Hall.
13. Fried, M. H. 1967. *The evolution of political society.* New York: Random House.

14. Morgan, L. H. 1877. *Ancient society.* Chicago: Kerr.
15. Oakley, A. 1973. *Sex, gender and society.* New York: Harper and Row.
16. Tresemer, O. 1975. Assumptions made about gender roles. In *Another voice,* eds. M. Millman and R. K. Kanter. Garden City, N.Y.: Anchor Press.
17. Fox, R. 1972. Alliance and constraint: Sexual selection in the evolution of human kinship systems. In *Sexual selection and the descent of man 1871-1971,* ed. B. G. Campbell. Chicago: Aldine.
18. Tiger, L., and Fox R. 1971. *The imperial animal.* New York: Holt, Rinehart and Winston.
19. Mead, M. 1935. *Sex and temperament in three primitive societies.* New York: William Morrow and Company.
20. Reiter, R., ed. 1975. *Toward an anthropology of women.* New York: Monthly Review Press.
21. Livingstone, F. 1968. The effects of warfare on the biology of the human species. In *War: The anthropology of armed conflict and aggression,* eds. M. Fried, M. Harris, and R. Murphy. Garden City, N.Y.: Natural History Press.
22. Braidwood, R. J. 1948. *Prehistoric men.* Chicago: Chicago Natural History Museum.
23. Peake, H., and Fleure, H. J. 1927. *Hunters and artists.* Oxford: Clarendon Press.
24. Kety, S. S., Rosenthal, D., Wender P. H., Schulsinger, F., and Jacobsen, B. 1975. Mental illness in the biological and adoptive families of adopted individuals who have been schizophrenic: A preliminary report based upon psychiatric interviews. In *Genetic research in psychiatry,* eds. R. Frieze, D. Rosenthal, and H. Brill. Baltimore: Johns Hopkins University Press.
25. Trivers, R. 1971. The evolution of reciprocal altruism. *Quarterly Review of Biology* 46:35-57.
26. Trivers. R. 1974. Parent-offspring conflict. *American Zoology* 14(1):249-64.
27. Huxley, J. 1932. *Problems of relative growth.* London: Macreagh.
28. Gould, S. 1966. Allometry and size in ontogeny and phylogeny. *Biological Review* 41:587-640.
29. DeVore, I., Goethals, G., and Trivers, R. 1973. *Exploring human nature.* Unit 1. Cambridge, Mass.: Educational Development Corporation.
30. Hamilton, W. D. 1964. The genetic theory of social behavior. Parts I and II. *Journal of Theoretical Biology* 7:1-52.
31. Foulks, E. F. 1975. Schizophrenia held useful for evolution. *The New York Times,* December 9, 1975, p. 22.
32. Lewontin, R. C. 1967. An estimate of the average heterozygosity in man. *American Journal of Human Genetics* 19:681-85.
33. Lewontin, R. C. 1972. The apportionment of human diversity. In *Evolutionary biology,* eds. T. Dobzhansky, M. K. Hecht, and W. C. Steere. vol. 6. New York: Appleton-Century-Crofts.

At the time that this article was written, the Sociobiology Study Group included L. Allen, J. Alper, B. Beckwith, J. Beckwith, S. Chorover, D. Culver, N. Daniels, E. Dorfman, M. Duncan, E. Engelman, R. Fitten, K. Fuda, S. Gould, C. Gross, W. Hill, R. Hubbard, J. Hunt, H. Inouye, T. Judd, M. Kotelchuck, B. Lange, A. Leeds, R. Levins, R. Lewontin, M. Lieber, J. Livingstone, E. Loechler, B. Ludwig, C. Madansky, M. Mersky, L. Miller, R. Morales, S. Motheral, K. Muzal, M. Nestle, N. Ostrom, R. Pyeritz, A. Reingold, M. Rosenthal, D. Rosner, H. Schreier, M. Simon, P. Sternberg, P. Walicke, F. Warshaw, and M. Wilson.

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THE FALLACY OF BIOLOGICAL DETERMINISM

by

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Reprinted from The Sciences, March/April 1976

The Fallacy of Biological Determinism

There's no good evidence suggesting that IQ is inherited

by R.C. Lewontin

Even the most superficial look at the history and present state of human societies shows an immense inequality in status, wealth and power both between individuals within societies and between nations and continents. This manifest inequality is in sharp contrast to the leading value in Western ideology since the 18th century, a value that characterizes the national myths of all the states of the world, the value of equality. If it is really "self-evident" that "all men (but apparently not women) are created equal" and that the political structure of states is designed to eliminate artificial barriers to equality and to promote general welfare, then how are we to explain those manifest inequalities in wealth and power between individuals and nations?

One possibility, of course, would be to assert that the political economy of most nations is not such as to promote equality in status, wealth and power, but, on the contrary, is carefully designed by the possessors of these attributes to keep themselves in possession. The idea that inequalities are a structural element of our social organization is not a popular one and not surprisingly is regarded with hostility by the governmental, educational and information-producing agencies of our society. The alternative, which has proved more palatable and, of course, more serviceable, is that our society is pretty much as fair as any society can be and that the inequalities we observe are the irreducible differences resulting from basic biological differences between people. This is, in effect, the ideology of *biological determinism*. It asserts that although some minor improvements may still be possible in spreading status, wealth and power, there is a limit to how closely equality can be approached—a limit set by the nature of the human species.

Biological determinism as an ideology has two seemingly contradictory facets, both of which are complementary and necessary to the complete argument. First, biological determinists claim that differences in status, wealth and power among individuals and among groups are the result of genetic differences in abilities, personalities and behaviors. Thus, some people are more aggressive, more intelligent, more creative, more skillful because, it is asserted, their genes make them so; if one nation or race has a greater preponderance of these superior genotypes, then that group will dominate as a whole. As Arthur Jensen puts it, some people can paint houses while others can paint pictures. "We have to face it: the assortment of persons into occupational roles simply is not 'fair' in any absolute sense. The best we can hope for is that true merit, given equality of opportunity, act as a basis for the natural assorting process." (*Harvard Educational Review* 39: 1-123, 1969)

But the ideology requires more than the view that differences in ability and personality are innate. It can be argued, as Dobzhansky did in *Social Biology* (20: 280-288, 1973), that the fact of biological diversity in abilities does not in itself necessitate social inequalities. We might simply organize our society so that both house painters and picture painters would receive the same psychic and material compensations. It is at this point that the second facet of biological determinism enters the argument.

Biological determinists argue that superimposed on the biological differences between individuals in ability is a tendency towards biological uniformity in behavior. Together, these biological qualities guarantee, in the determinists' view, that societies will always be hierarchical. The argument of a biologically based *human nature* holds that man tends to be "by nature" aggressive (Lorenz, 1963), territorial (Ardrey, 1966), entrepreneurial, genocidal, indoctrinable, male dominated (Wilson, 1975). That biological differences in ability between individuals must inevitably be turned into a hierarchical system of differential status and wealth is nowhere better stated than in Wilson's description of the characteristic political economy of the human species: "The members of human societies sometimes cooperate closely in insectan fashion, but more frequently they compete for the limited resources allocated to their role-sector. The best and most entrepreneurial of the role-actors usually gain a disproportionate share of the rewards, while the least successful are displaced to other, less desirable positions." (*Sociobiology: The Modern Synthesis*, Harvard University Press, Cambridge, Massachusetts, 1975)

This description, it must be noted, is not regarded as a historically contingent phenomenon in market societies, but as arising out of the biological nature of the human species. A human society like that envisaged by Dobzhansky, in which genetic differences in ability are not converted into status and wealth differences, would be biologically "unnatural" for Wilson and therefore either impossible or maintainable only under the most rigid totalitarian rule.

The attempt to justify the status quo by a resort to biological arguments has a long history. Before the middle class revolutions of the 18th century, aristocrats claimed superior "blood." In the 19th and 20th centuries, such arguments became more sophisticated, first turning to anatomy and then to DNA as a basis for the biological dis-

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tinction. Thus, in the 19th century Lombroso founded an influential school of "criminal anthropology" with his claim that "criminal types" could be recognized by the conformation of their ears, noses, skulls, lips, etc. No less a personage than Louis Agassiz, the greatest of 19th century American zoologists, claimed that the skull sutures of black children closed earlier than those of whites. Consequently, it was dangerous to teach blacks too much for their brains would swell up and burst inside their rigid crania. Although this may seem absurd, it is typical of the claims of 19th century physical anthropology and comparative anatomy, which found blacks, criminals and women to be either more childlike, more primitive, or less developed anatomically than northern European men.

administered at Ellis Island and the Army classification tests given to those inducted during the first World War, had shown most southern and eastern European immigrants to be mentally inferior. Supported by the overwhelming testimony of the American academic community, with virtually no dissenting voice from geneticists, an immigration act was passed by Congress setting national quotas for immigrants proportional to the numbers of those nationalities in the United States as of the 1890 census, when the overwhelming majority of Americans had come from northern Europe. The Immigration Act of 1924 was the greatest political consequence of biological determinism until that time, but it was soon to be surpassed by the deaths of "biologically inferior" millions in eastern Europe.

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After the rediscovery of Mendel in 1900, the gene took over as the biological basis for human differences. At about the same time, intelligence testing was founded in France by Binet and was imported into the United States and popularized by Terman; his 1916 Stanford-Binet test was the prime tool of the mental testing movement in the United States. The idea that intelligence could be measured quantitatively and the rapid advances in Mendelian genetics came together to produce the single most powerful and popular form of biological determinism, the eugenics movement. It was asserted that the health and welfare of society were dependent upon the more intelligent people, that intelligence was determined chiefly by genes, that the less intelligent were producing too many children and that, by both law and custom, this degeneration of the population should be stopped and reversed.

A racist version of the eugenics doctrine held that blacks, orientals and southern and eastern Europeans were genetically inferior mentally and morally, as demonstrated by intelligence testing. This aspect of the eugenics movement culminated in the U.S. Immigration Act of 1924. Until that act was passed, immigration of Europeans had been unrestricted. It was argued by mental testers, including such founders of American educational psychology as Terman, Yerkes and Goddard, that IQ tests, especially tests ad-

Although the shock produced by Nazi racism resulted in a disappearance of biological determinist arguments from respectable intellectual circles for twenty years, these arguments have now come back into fashion among academics, albeit in more sophisticated and "scientific" forms. The latest versions include the claims that 1) differences between individuals in school and socioeconomic success are chiefly the result of genetic differences and are therefore unchangeable (Jensen, 1969); 2) differences in average scholastic and socioeconomic levels among races (chiefly black and white) are mostly genetic and therefore unchangeable (Jensen, 1969, 1975); 3) differences between social classes are mostly the result of genetic and unchangeable differences in intelligence (Herrnstein, 1971); 4) differences in status and roles of men and women are largely biological, a result of the evolution of our species, and are therefore natural and unchangeable (Tiger and Fox, 1971; Wilson, 1975); 5) as mentioned earlier, the human species has built into its genes as a result of its evolutionary history such traits as aggression, territoriality, conformism, indoctrinability, entrepreneurship, etc.

The arguments about innate differences between sexes and about "human nature" are of a speculative and loose form appealing chiefly to common sense notions and prejudices, to analogies with non-human animals, and to

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carefully selected data from paleontology, archeology and ethnography. There is not a great deal that can be said about them except to remind the unwary reader that they are purely imaginative invention and speculation, even though they are often presented as demonstrated fact. The arguments about genetic differences in ability among individuals and groups, however, have a much greater appearance of "objectivity," since they use statistical analyses, cite masses of evidence from quantitative tests of intelligence of relatives of various degrees and, in general, bring into play what appears at first sight to be an unchallengeable apparatus of statistical-genetical theory and observation. "Facts are facts," we are told, "and even though we may not like the conclusions, we must be objective about them."

It turns out that on close examination the concepts on which the determinist position is based prove to be incorrect, the theoretical arguments offered do not stand up logically, and the "facts" in some important cases have been cooked. Therefore, for the remainder of this article, I shall look at that particular brand of biological determinism embodied in the claims of hereditarians that differences in performance on IQ tests are chiefly genetic and unchangeable, and that differences between races and social classes in performance on these tests are also genetic and unchangeable.

I will leave aside the questions of whether performance on IQ tests measures anything that can be called "intelligence" in an important sense and whether performance on such tests is, in fact, a good predictor of eventual socioeconomic success. The first of these questions seems to me entirely metaphysical, and the answer to the second depends upon what one means by a good predictor. The gross correlation between IQ and eventual socioeconomic success, as measured on the scores used by sociologists, is rather poor (only about 25 per cent of the variation in socioeconomic success is explained by variation in IQ) and that correlation decreases considerably if such variables as family background and years of schooling are held constant. In fact, these latter variables explain much more of the variation in socioeconomic success than does IQ, but at present no set of variables gives very high predictability. (Bowles and Nelson, 1974, *Review of Economics and Statistics*, 56: 1) I pass over this point lightly because my main concern is with the misuse of biology.

The main *conceptual* error in the biological determinist argument stems from an incorrect understanding of the relation of gene, environment and organism. The error appears in different forms, sometimes naive and simplistic, sometimes sophisticated. Jensen's 1969 paper in *Harvard Educational Review* (39: 1-123) contains all the forms. The title of the paper is "How much can we boost IQ and scholastic achievement?" The answer given in the paper is "not much" and the reason given for the answer is that IQ differences are mostly inherited. This is the first and most elementary fallacy of biological determinism.

It is totally incorrect to equate "inherited" or "genetic" with "unchangeable," although this equation is often made. The plasticity of a morphological or physiological or behavioral trait is not made greater or less by the fact that genes influence it. If "inherited" meant "unchangeable," then human clinical genetics would be a largely fruitless enterprise, limited only to counseling parents about the risks of having defective children. But that is not the case. Inherited disorders can be treated and corrected as easily (or with as much difficulty) as those arising from birth traumas, accidents or environmental insults. To cure a disorder, we need to know what the metabolic or anatomical lesion is, not whether it is the result of being homozygous for a gene.

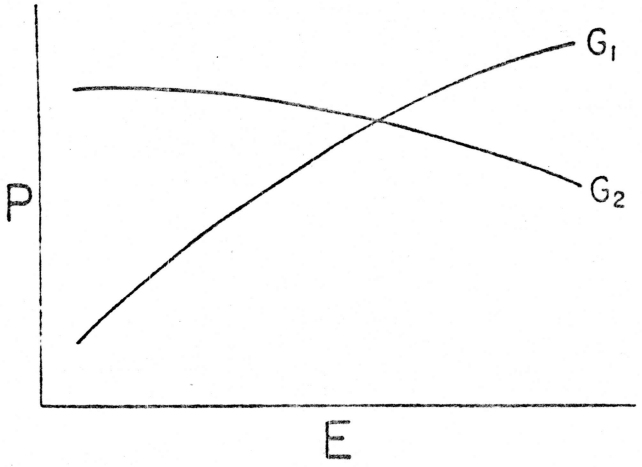
It is irrelevant whether "blue babies" suffer from a genetic or accidental development disorder; the cure is the same—surgical intervention to close off the short circuit in the circulation. Wilson's disease is characterized by progressive degeneration of nervous and other tissues resulting in death in late childhood or early adulthood. It is the result of being homozygous for a single recessive mutant gene which, as it turns out, causes the sufferer to retain too much copper in his body. This knowledge of the metabolic lesion has led to prevention of the degenerative process through the use of a simple drug that removes excess copper ions.

What is true for extreme disorders is also true for the normal. Whether behavior is modified by social influences depends upon events that long postdate the original action, if any, of genes influencing the growth of neurons. Genes do not mediate processes that are somehow sealed off from the rest of the universe. As Jacob and Monod showed long ago, even the primary transfer of information from gene to protein is directly modifiable by information from the environment. The opposition of "inherited" and "modifiable" is a relic of 19th century thought.

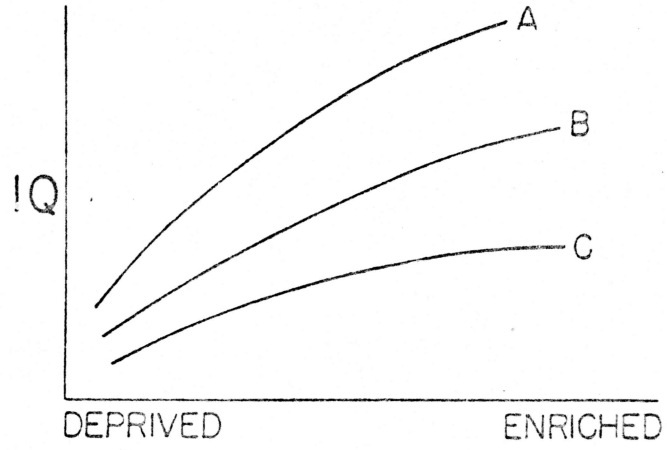
A more sophisticated form of the erroneous view of gene and organism is that of "genetic potential," also found in the writings of Jensen and especially in those of Herrnstein. Although the concept admits that gene and environment interact, the description of that interaction incorrectly assumes that the genes set the limit or the maximum of a character, while the environment determines how much of that "limit" will be realized or fulfilled. It is as if we were empty buckets at birth, some larger than others. If the environment is poor, even the large buckets will be only partially filled; if the environment is enriched, every genotype will reach its maximum and the large buckets will contain more than the small ones. But there is nothing in developmental genetics to sustain this idea of differing genetic potentials.

To understand the situation, we must look at the concept of the *norm of reaction*, the most basic concept of developmental genetics. For a particular genetic type, there is some functional relationship between the environment experienced by the developing organism and the resulting morphology, physiology or behavior that is developed, the so-called phenotype. The norm of reaction of a given genotype is the relation between its phenotypic expression and the environment in which it has developed. Different genotypes have different norms of reaction. Figure 1 shows two hypothetical genotypes with different norms of reaction—one, G_1, in which the phenotype, say weight, increases with the environmental variable, say temperature, while the other, G_2, has a decreasing weight with increasing temperature.

In order to draw the norms of reaction for different genotypes, it is necessary to allow each genotype to develop in a series of different environments and to observe the resulting phenotypes. But such an experiment is quite impossible for any human trait, especially any human behavioral trait, because it is simply impossible to produce a large number of human beings, all of whom have the same genetic constitution, and then to raise each one in a different controlled environment. We would need large numbers of groups of "centuplets" and dictatorial power to put each in a different environment, obviously an absurdity. For that reason, *no one has ever been able to characterize human norms of reaction for any behavioral trait* (or for any human trait at all). Where norms of reaction for different genotypes have been determined in experimental animals, they tend to look like Figure 1. That is, they cross each other, with one genotype having a larger phenotype in some environments and another genotype having a larger phenotype in other environments. Thus, it is clearly impossible to speak of one genotype having a higher "potential" than the other which is "fulfilled" in a "better" kind of environment.



Despite the fact that the norms of reaction that are known tend to look like Figure 1, and despite the fact that human behavioral norms of reaction are *totally unknown*, Jensen in his 1969 paper discusses the concept of norm of reaction using the lines shown in Figure 2. These hypothetical lines correspond to the erroneous notion of genetic potential. Genotype A is better than B, which is better than C in all environments. Thus, enriching the environment will not help C relative to A. On the contrary, the lines are drawn so as to fan out in the "enriched environment." The obvious implication is that the genetically superior types will be even more superior as the environment is made better. The conclusion one is supposed to draw is that spending more money and effort in general improvements in education will only produce greater inequalities than already exist between genotypes. But there is not a shred of evidence for such a proposition as Figure 2. It is simply a convenient hypothesis arranged to suit an erroneous preconception.



The third, and most sophisticated, form of hereditarian error is the use of the concept of *heritability* in describing the degree to which genes influence behavioral traits. It is not possible to ascribe a particular fraction of an individual's phenotype to genes and the rest to environment—it is an obvious absurdity to say that of my 5 feet 11 inches of height, I owe 5 feet to my genes and 11 inches to my environment. It is possible, however, to divide up the *variation in a population* into a variation that results from genetic variation and one that results from environmental variation. The proportion of all the variation that is genetic is called the heritability of the trait, H^2, in the population. Symbolically, H^2 = genetic variation/genetic variation + environmental variation.

As defined H^2 applies only to a particular population at a particular time, since another population at another time will have different genotypes and different environments. Thus we cannot say that "the heritability of weight is 43 per cent," but only that "in the population of New York, in 1976, the heritability of weight is .43 per cent." Yet in discussing IQ, biological determinists repeatedly refer to the heritability of IQ as a constant, usually giving 80 per cent as their best estimate. What they fail to understand (or willfully ignore) is the fact that the per cent heritability applies *only* to the set of environments in which the determination was made and cannot be extrapolated to other distributions of environments.

More important than the error of supposing the heritability of IQ to be a universal constant is the incorrect implication drawn from the figure. If the heritability of IQ is 80 per cent, it is asserted, this means that IQ is "mostly genetic" and therefore not very changeable, or at least that the variation among individuals in IQ cannot be reduced by more than about 20 per cent since 80 per cent of the variation is genetic. But, as I have pointed out, this is totally incorrect. The heritability of a trait gives us no hint as to how malleable or plastic the trait may be. Before the discovery of the faulty copper metabolism that underlies Wilson's disease, it was 100 per cent heritable. It is simply a non sequitur

to claim that if the heritability of IQ were 80 per cent, then only 20 per cent of the variation could be removed by varying the environment.

The error is compounded by biological determinists, who go on to claim that the heritability of IQ *within* populations is a basis for concluding that the differences *between* groups are also genetic. Jensen commits this error with respect to black-white differences and Herrnstein does it with respect to social class differences. But there is neither a logical nor empirical relationship between the heritability of a trait within a population and the causes of differences between populations. Differences within and between populations arise from quite separate causes on quite different time scales and one cannot be judged from the other. So, for example, the difference in skin color among New Yorkers classified as "white" can reasonably be attributed to heredity, since the Finns, Italians, Slavs, Puerto Ricans and so on clearly differ in skin pigmentation genetically. Yet no one would assert that the difference in skin color between New Yorkers who stay in New York all winter and their better-off relatives who spend the winter in Miami is genetic.

Differences within populations are a manifestation of the genetic history of the population in recent times, especially of migrational mixture, and of environmental differences of a local nature having to do with the internal organization of the society. Differences between populations reflect long term genetic divergence and major environmental differences associated with the clear identification and separation of the groups. There is simply no way to judge the causes of between group differences from genetic variation within groups. Although this point has been made over and over since Jensen published his paper in 1969, it does not seem to have been understood, since the error reappears in his 1975 book. (*Educability and Group Differences*, Harper and Row)

Finally, let us turn to the "facts" from which the heritability of IQ has been calculated. It is worth looking at the status of some of these facts, not because they themselves are relevant to any social issues, but because the gathering and presentation of these facts have been so scandalously bad as to constitute a veritable Watergate of human behavioral genetics.

The study of genetics is the study of relatives. If genes influence a trait, then closer relatives should resemble each other, on the average, more than distant relatives. This study of relatives presents no problem in experimental organisms, where related individuals can be placed randomly in different environments. In the human species, the problem is to distinguish similarities that arise from genetic relationships from similarities that arise from environmental correlations, since close relatives tend to share environments as well as genes. Any estimate of heritability will then be upwardly biased by an unknown amount, unless the relatives being compared have been randomized across environments. That is, we must place children of the same biological parents with adopting parents who will represent a random sample of the population with respect to various socioeconomic variables that might influence intelligence.

Despite this elementary consideration, the first lesson in any course in biometrical genetics, *the great bulk of estimates of heritability of IQ come from a comparison of relatives raised together in the same family*. Thus, these estimates are completely useless. Although this kind of error is so elementary that no student in an animal breeding course could make it without being called to account, behavioral geneticists continue to publish such estimates of heritability in the leading journals of their field with the approval of the editors and referees who are their colleagues.

But gross experimental errors are only a part of the story. Realizing the importance of adoption studies, those who have attempted to estimate heritability of IQ from published studies of identical twins raised apart and together have relied heavily on four such studies. The largest of the four studies comparing twins raised apart and together was the series carried out by Cyril Burt and his colleagues; the Burt studies are a major source of the 80 per cent heritability of IQ so often quoted. It now appears, however, that Burt's "data" are not part of the corpus of science at all, but of science fiction. In 1973, Kamin, addressing the American Psychological Association, showed so conclusively that Burt had cooked his data in at least two ways that Jensen was forced to conclude that they could not be relied upon. (*Behavior Genetics*, 4:1-28, 1974) Kamin has since published his brilliant detective work as *The Science and Politics of IQ*. (Lawrence Erlbaum and Associates, Potomac, Md., 1975)

First, it appears that Burt did not use actual test scores in his comparisons, but made "final assessments" subjectively and altered the numerical scores to be more in accord with his hypothesis of genetic determination. Not content with these adjustments, Burt further "adjusted" his statistics to give the following remarkable results: in 1955, with 21 twin pairs raised apart and 83 pairs together, the correlation for twins raised apart was .771 while for those raised together it was .944. In 1958 with more twin pairs added to the tests, the correlations were again .771 and .944. In 1966, with still more pairs—53 raised apart and 95 together—the correlations were still .771 and .944! Sir Cyril Burt must have been a remarkable experimenter indeed.

Nor is Burt the only sinner. Kamin's book reveals several other instances of the plotting of non-existent data points, the suppression of others, etc. Ignorance and inadvertance cannot be pleaded here.

How are we to explain the scandalous state of investigations in this field? It is hard to come to any other conclusion than that it is the result of the explicitly political position of the advocates of biological determinism. Not content with making conceptually incorrect and contradictory assertions about what might be the case if IQ were highly heritable, they have tried to force the data to fit their preconceptions. In this act of supererogation they have given themselves away.

All of science, of course, has an ideological component. No research is entirely free of the social and political views of its practitioners. The very act of engaging in experimental science in an academic context carries with it a host of political presuppositions about the allocation of labor, resources and power in society. But occasionally ideology becomes so overriding and political advocacy so potent as to completely dominate the mode of scientific production.

As we look back at the discredited versions of biological determinism in the 19th century and in the first half of the 20th, they seem to us crude and sometimes absurd. Who would be so foolish as to believe that one could tell a murderer from the shape of his skull? Yet we are asked seriously to believe in 1976 that the possession of an extra Y chromosome predisposes people to criminal acts. The arguments for the heritability of IQ are just another instance of the same old ideology in modern and respectable guise. Nothing is known in biology that dictates the limits of human social organization or individual fulfillment. Claims to the contrary must be regarded with extreme suspicion, for they usually turn out to benefit their authors. ☭

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A METHODOLOGICAL ANALYSIS OF SOCIOBIOLOGY

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I

Since the publication of Prof. E. O. Wilson's book Sociobiology: A New Synthesis[1] in 1975, the field of sociobiology has captured the imagination of much of the general public as well as of the academic world. At the time of its publication the book was proclaimed by The New York Times as marking the birth of a new science.[2] Subsequently, magazine articles, interviews with Wilson, articles in biological journals, articles in journals in the social sciences, symposia conducted by various academic disciplines, a high school curriculum entitled "Exploring Human Nature," and even a movie featuring interviews with Wilson and two other prominent sociobiologists have all been devoted to descriptions, discussions, and sometimes debates on sociobiology.[3] This enormous interest no doubt derives from the ambitious goals of the subject: sociobiology claims to be able to explain the social behavior of animals including humans in terms of underlying evolutionary biological principles. In Wilson's words, "The ultimate goal is a stoichiometry of social evolution" (p. 63).

In his vision of the future of the biological and behavioral sciences, Wilson surmises that in the next hundred years or so cultural anthropology, social psychology, and economics will be incorporated into sociology. An understanding of emotions and ethical judgements will be based on a full neuronal explanation of the human brain at the level of the cell. "Cognition will be translated into circuitry" (p. 575). "Having cannibalized psychology, the new neurobiology will yield an enduring set of first principles for sociology" (p. 575). Evolutionary sociobiology will elucidate the biological origins of the machinery (Wilson's term) of human behavior and thus predict which sets of behavior can be changed without resultant emotional damage. The theory would also enable scientists to monitor the genetic basis of human behavior in order to determine what sort of planned society is compatible with biological evolution.

It is evident that this extreme reductionist program is of great interest to biologists since it claims to open up whole new areas of study. It is of interest to social scientists both because of the claims that their fields are to be "cannibalized" and more positively, because some social scientists see the introduction of sociobiological concepts as strengthening the "scientific" foundations of their disciplines and according them greater legitimacy. Finally sociobiology is of general interest because of its political implications. A biologically based theory tends to imply that some behavior is natural and consequently that there are grave risks inherent in attempting to change the structure of a society based on this natural behavior. For sociobiological theorists, the natural order bears a striking resemblance to contemporary Western capitalist societies characterized by entrepreneurship, sexism, and aggressive behavior. Thus the present theory carries with it strong justifications of the status quo.

In this paper we undertake a study of the methodology of sociobiology and discuss its status as a scientific theory. We shall discuss first the reductionist aspects of the theory and then its logical structure in order to show how the sociobiologists attempt to cast their theory in the mold of theories in the physical sciences. Sociobiology, however, cannot be justified by means of traditional scientific standards of evidence and as a result other methods must be used to legitimate the theory. In view of the discussion of the methodology of sociobiology and the techniques used to justify it, sociobiology will be seen to take the form of an ideology which can be used to explain, rationalize and thus legitimize our society. We conclude this paper by examining sex roles in the theory as an example of its ideological implications and show how the sexism in our society can be justified by sociobiological arguments.

This paper in no way exhausts the methodological problems inherent in sociobiology. For example, the reification of behaviorial constructs, the origins of the categories used in sociobiological descriptions of behavior, the use of homology and analogy, and the conflation of adaptive and genetic arguments are not treated in any detail here. We have chosen instead to concentrate on what we consider to be the more fundamental problem of the logical structure of sociobiological theory.

At the outset, it is useful to outline the formalism of the theory especially as it is applied to the human species. Sociobiological theory proceeds by first identifying certain behavioral traits that are deemed to be universal among all peoples. It is then postulated that these traits are biologically determined, meaning that differences in these traits from one person to another are primarily due to genetic rather than environmental factors and also that these traits would remain basically unchanged no matter how the environment was changed. It is further assumed that these traits are adaptive, i.e., that possession of these traits results in a greater probability of surviving until reproductive age and/or a greater number of surviving offspring. Practitioners of sociobiology are concerned with identifying the universal traits, searching for evidence that they are genetic, and providing explanations of their adaptive value. Human social behavior is explained as a result of biological factors and can be understood only be means of the study of biology.

The acrimonious debates that sociobiology has generated are largely due to its political implications. Various critiques of the theory have pointed out that sociobiology is not value-free, objective science and that the political implications are not simply possible interpretations and extensions of the theory but rather are inherent in it.[4] In focusing on the hypotheses of

the theory, these critiques have shown that the choice of traits deemed universal by sociobiologists, e.g., entrepreneurship, male dominance, and indoctrinability, reflect historical conditions and introduce a set of values into the theory. Many of these traits are in fact not common to all human societies. Furthermore, many of the analogies made between animal behavior and human behavior designed to prove that human behavior is as biological as animal behavior are flawed. The hypothesis that these traits are genetic implies that they correspond to some external biological reality. Sociobiologists thereby commit the error of reification by assuming that theoretical constructs describing complex historically conditioned behavioral patterns are biological entities coded for by the genes. In addition there is no evidence that any human social behavior is genetically controlled. Finally the hypothesis that all traits have adaptive value has been rejected by modern evolutionary theorists, and so there are no biological reasons to support the contention that behavioral traits will persist because of evolutionary pressures.

II

We now turn our attention from the hypotheses of sociobiology and examine the nature and structure of the theory as a whole. As Wilson points out, theories can be divided into two categories, phenomenological and fundamental (p. 27). A phenomenological theory is one that consists of explanations or mathematical equations that describe some set of experimental data (the phenomena). The ideal gas law of physical chemistry, which is an equation specifying the relationship among the pressure, volume, temperature, and quantity of a gas, is an example of a phenomenological law. "The true role of fundamental theory," on the other hand, "is not to confront the raw data, but to explain the relatively few parameters of the phenomenological theory in terms of which the great mass of raw data has been analyzed."[5] Statistical mechan-

ics is such a fundamental theory from which, using as fundamental entities the mass of a gas molecule and the volume of the container, the parameters of temperature and pressure can be explained and the ideal gas law can be derived. Probably the greatest achievement in the physical sciences has been the ability to analyze phenomenological theories in widely differing subject areas and then to create a fundamental theory to explain the phenomenological parameters and derive the phenomenological laws that describe the various experimental observations. For example, Newton's theory of mechanics and gravitation is a fundamental theory that describes the laws of motion of such diverse objects as cannonballs on earth and the planets in the heavens using the single parameter of the mass of the object.

This process of finding fundamental theories from which phenomenological theories can be derived is called reductionism.[6] The term reductionism is usually used to refer to a hierarchy of sciences, e.g., physics to chemistry to biology to psychology; and to the "reducing" of each science on this list to the concepts and theories of the science preceding it. The aim of reductionism is to reduce any phenomenological theory to the minimum number of parameters by deriving or explaining all the other parameters using the fundamental theory. Such a program can be successful only if all the complexities of any given theory can in fact be explained by using fewer parameters with no loss in the richness of the theory that has been reduced. Newton's laws are only one example of successful reductionist science. Maxwell's equations which explain nearly all classical electro-magnetic phenomena and quantum mechanics which in principle reduces all of chemistry are two others. However, even in the physical sciences, the extent of the reductionist program's applicability is still an open question. For example, the success of the fundamental theory of statistical mechanics in reducing the phenomenological theory of thermodynamics

in such a way as to incorporate "time's arrow," the unidirectionality of time, is still a subject of lively debate among physicists and philosophers. It goes without saying that the possibility of reducing life to the more fundamental theory of chemistry is a completely open question. Of course, the failure of the reductionist program would not mean that life processes do not obey the laws of physics, but only that the laws of physics may not be sufficiently rich to encompass all of the complex interacting processes that are involved in what we call life.

The difficulties inherent in a reductionist program for a set of phenomena characterized by multiple and complicated interactions become extremely serious for such a field as sociobiology. Wilson expresses his view of reductionism in sociobiology as follows:

> Its central precept is that the evolution of social behavior can be fully comprehended only through an understanding, first, of demography, which yields the vital information concerning population growth and age structure, and, second, of the genetic structure of the populations... The principal goal of a general theory of sociobiology should be an ability to predict features of social organization from a knowledge of these population parameters combined with information on the behavioral constraints imposed by the genetic constitution of the species. It will be a chief task of evolutionary ecology in turn, to derive the population parameters from a knowledge of the evolutionary history of the species and of the environment in which the most recent segment of that history unfolded (p. 5),

and more specificially

> In general, phenomenological theory aims at equations that predict the quantitative data of demography and territory size, the ecological and physiological correlates of dominance hierarchies, role differentiation and other features of social organization. Fundamental theory attempts to derive these equations from the first principles of population genetics and ecology (p. 28).

Wilson's evident confusion about the role of ecology both as fundamental theory and as phenomenological parameters underlines the difficulties in his reductionist program. Ecology is the science which studies the interactions

and relationships of animals with their total environment. Since the behavior of animals modifies their environment in addition to being affected by it, the uni directional logical chain that Wilson proposes from "evolutionary and ecological parameters" to a "theory of evolutionary ecology" to "behavioral and population parameters" and finally to "the theory of sociobiology (social behavior of animals)" seems inappropriate (p. 5, Fig. 1.1). In fact, the whole notion of "ecological parameter" may be invalid, since a parameter is generally defined as an independent variable whose range of values is independent of the theory (except in so far as the values of the parameter must remain within the domain of validity of the theory). Since the ecological parameters depend on the entire theory of evolutionary ecology, both evolutionary history and the present behavior of animals, it is difficult to see how ecological parameters can be used as a well-defined set of parameters in sociobiological theory.

One possible resolution of this problem might be to argue that ecological considerations provide the "boundary conditions" for the fundamental theory of population genetics. By boundary conditions we mean the specific empirically determined state of the world (or domain of the theory) at a particular time. In such a situation, the dynamical laws of motion which control social organization, to use mechanistic terminology, would arise entirely from population genetics. The difficulty still remains, however, that the ecological conditions are time dependent, and it is by no means obvious that this time dependence is governed soley by the dynamics provided by population genetics. In the language of the physical sciences, such a solution entails a Markoff model in which the behavior of a system is dependent on the state of the system at <u>one</u> past time only. Since, as Wilson himself acknowledges in the passages quoted above, the state of an ecological system is dependent on its past history, the values of the ecological parameters are highly dependent on the history of the system.

For these reasons there is no escape from the requirement of a fundamental theory of ecology which could include historical considerations.

These problems all stem from the reductionist goals of the theory. Ecology and sociobiology deal with animals, their environments, and all attendant interactions including animal behavior. Evolutionary ecology and sociobiology are not sequential disciplines like physics and chemistry; their subject matter cannot be cleanly separated. To achieve any kind of meaningful understanding, the whole system, the animals and their environment must be solved "self-consistently," that is, all at once, rather than by postulating some initial parameters of ecology or population genetics and expecting to derive the consequent behavior by means of determinant laws of evolution.

The difficulties increase exponentially for the sociobiology of the human species. Cultural history and the present activities of people play a vital role in our behavior and cannot be disentangled from our biological environment. Our social behavior is conditioned not only by our physical and biological surroundings, as it is for all other animals, but it is also heavily influenced by our consciousness of our past and present behavior and our ability to use that knowledge to modify our future behavior. The number of interactions and feedback loops created in these processes is mind boggling, and the possibility of reducing human behavior to sociobiological parameters derived from fundamental theory appears to be illusory.

III

In his book, Wilson repeatedly emphasizes that the theory of sociobiology is in accordance with the traditional canons of scientific methodology, and contrasts the theory with what he terms "the advocacy method of the developing

sciences" (p. 28). Earlier biological determinists such as Lionel Tiger, Robin Fox, Robert Ardrey, Desmond Morris, and Konrad Lorenz all, in Wilson's view, employed this advocacy method by first proposing hypotheses to account for a certain set of phenomena and then marshalling the evidence needed to make a persuasive argument. In the theory of sociobiology, on the other hand, "it has been possible to derive propositions by traditional postulational deductive methods of theoretical science, and test many of them rigorously by quantitative study."[7]

The postulational deductive method consists of the following procedure: (i) Postulate a theory (let us call it T) to account for some set of phenomena. (ii) Specify some particular state (p) which is either observable in nature or is realizable experimentally. (iii) Deduce the consequences of p which follow from the application of the theory. We call the consequent state q and require that it too be experimentally observable. (iv) Confirm experimentally that if p occurs then q occurs. To be more specific we call this procedure the unilateral postulational deductive method. By contrast, we define a bilateral postulational deductive method requiring two additional steps: (v) Apply T to the negation of p and deduce the negation of q. (vi) Confirm experimentally that if p does not occur, then q does not occur. Steps (v) and (vi) describe the procedure of controlled experiments.

The empirical sciences have relied primarily on the bilateral type of postulational deductive method because of a serious flaw in the unilateral method. The unilateral method shows only that the empirical world is compatible with the theory. As an example consider a primitive group of people who perform a special dance during solar eclipse in order to make the sun reappear. All the conditions of the unilateral method are fulfilled. T is "the dance causes the sun to reappear upon the eclipse;" p, "the dance is performed;" and

q, "the sun reappears." In order to confirm their theory by the bilateral method, it would be necessary for the group to realize the conditions of not-p, "do not do the dance." Only is not-q results, i.e., "the nonreappearance of the sun," would the theory be confirmed.

Wilson himself warns the reader of this Fallacy of Affirming the Consequent and gives several examples of its occurrence in the literature of animal behavior. He explains that "[t]he way around the fallacy is to devise competing hypotheses such that all but one can be decisively defeated" (p. 29). It is therefore ironic, to say the least, that Wilson's arguments, especially as applied to the sociobiology of humans, are replete with examples of this Fallacy.

The reliance of sociobiologists on the unilateral postulational deductive method is probably inevitable given the structure of the theory which automatically excludes important possible competing hypotheses. Almost by definition, sociobiology focuses its attention on the genetic (biological) basis of social behavior. Although it is generally accepted that genetic programming is the major determinant of (nonhuman) animal behavior unless compelling evidence shows otherwise, there is increasing evidence of the amount and degree of learned behavior among animals. The mastery of songs by various birds is crucially dependent on the young birds being able to imitate the songs of mature birds. And the emergence of primatology as a subdiscipline of anthropology in university curricula is an indication that primate behavior is now considered to be closely related to human behavior. For humans the transmission of knowledge of social behavior proceeds primarily through cultural rather than biological transmission. As Dobzhansky has said, "Culture is not inherited through genes, it is acquired by learning from other human beings... In a sense human genes have surrendered their primacy in human evolution to an entirely new nonbiological or super-organic agent, culture."[8]

Despite the protestations of sociobiologists that they recognize the importance of cultural factors and that genetics plays only a small role in human social behavior, sociobiology does not incorporate within it cultural and historical factors. The avowed program of sociobiology to base the study of social behavior on the fundamental theories of population genetics and possibly evolutionary ecology, precludes, or at least relegates to a subordinate role, the introduction of cultural factors. Inclusion of cultural factors would require an independent fundamental theory of the development of social structures. Depending on ones politics, such a theory might be based on, for example, such factors as the means and modes of production of the material aspects of human life, the preservation of this material life as well as the reproduction of its members, various aspects of alienation, and would analyze these factors historically. For sociobiologists, sociology, social psychology, and economics (or their eventual merged successor) remain phenomenological studies while neurophysiology (the direct physiological consequences of the actions of genes) and evolutionary biology remain as the only fundamental theories (p. 574-75).

Not only is there no developed theory of cultural transmission or of possible interactions between biological and cultural factors in sociobiology, but there is no mention of the need of such a theory. It is in this sense that sociobiology is an extreme hereditarian theory. Wilson maintains that human social behavior lies somewhere between the extremes of the classical biological determinism of, for example, Lorenz, and the radical environmentalism of B. F. Skinner, and that to find out "just where is what sociobiology is all about."[9] However, since rival theories of cultural transmission are not discussed, Wilson is unable to carry out his program. In no concrete case in the chapter on human sociobiology does Wilson treat genetic and cultural transmission as rival

theories, and produce empirical evidence to "decisively reject one." Instead he postulates arbitrarily one gene or several genes for each human behavioral trait and then deduces the results of the application of the evolutionary theory of natural selection. If the conclusions fit specific facets of empirical societies, he regards this as evidence of the truth of the hypotheses. Such an approach could serve as a model example of the unilateral postulational deductive method.

One of the more amusing examples of sociobiological reasoning concerns the explanation of the persistence of male homosexuality. Assuming with Wilson (even though there is little evidence for the assumptions) that homosexuals have fewer offspring than exclusively heterosexual males and further that there is probably a genetic predisposition toward this condition, we may wonder why it was not eliminated by the process of natural selection. Wilson offers two possible explanations. First, it may be that some heterosexual males carry the homosexual gene (or genes) and these carriers are more evolutionarily fit than noncarriers. Second, the homosexuals who are free from parental duties are able to help their immediate relatives (some of whom are carriers of the homosexuality gene) and thus these relatives are better able to survive and reproduce. As a result of either mechanism, homosexuality genes are propagated from one generation to the next. The second mechanism, which is used to explain altruistic behavior in animals, is called kin selection, since the trait in question increases the fitness of kin rather than of the individual animal itself.

It is obvious that in his choice of theories, Wilson ignores the equally plausible possibility that sexual preference is not genetically determined but is a purely cultural phenomenon. Such a hypothesis must surely be included if we are to be able to distinguish "decisively" between theories. Furthermore

we should note that Wilson's explanations do not even satisfy the conditions of the unilateral postulational deductive method. The initial experimental condition we have called p is in the first explanation, "carriers of the homosexuality genes are evolutionarily fitter," and in the second, "the homosexual male functions as a helper of his relatives." Neither of these conditions can be confirmed experimentally as is required by the method. Thus Wilson must qualify his arguments with such a statement as "if such genes really exist, they are almost certainly incomplete in penetrance and variable in expressivity" (p. 555).

The dependence of sociobiology on the unilateral postulation deductive method leads to a reductio ad absurdum: any conceivable social behavior can be shown to be adaptive and thus explainable in biological terms. In explaining everything, human sociobiology succeeds in explaining nothing and has no predictive value. Sociobiology becomes an untestable theory since none of its propositions can fail to be confirmed by experiment. We note parenthetically that this statement about untestability stands no matter what might be one's views of Karl Popper's falsifiability criteria for scientific theories. Let us now examine the features of the theory that result in this situation.

In addition to ordinary individual selection (evolutionary fitness of those individuals possessing traits best adapted to their environment) and kin selection (enhanced fitness of the genes of an individual by increasing the fitness of that individual's relatives), sociobiologists assume the existence of reciprocal altruism. An individual may help increase the fitness of an unrelated individual in the expectation that the latter individual will reciprocate at some future time. Such behavior is adaptive since it enhances the survival of both the recipient and the donor (who is a potential recipient). Finally, sociobiologists assume that spiteful behavior (behavior that benefits neither of the interacting

individuals) is both genetic and adaptive. For example, lying to others may be adaptive since people "typically do so in a way that deliberately diminishes outsiders while promoting relatives, even at the risk of their own personal welfare" (p. 119). By judicious use of the mechanisms of individual selection, kin selection, reciprocal altruism, and spite a just-so story can be invented to explain the existence of any conceivable social behavior.

It is well known, however, that changes in social behavior occur much more rapidly than can be accounted for by fluctuations and shifts in the frequencies of the appearances of certain genes. To save the phenomena, a multiplier effect is introduced. This effect, borrowed from economics, hypothesizes that small variations in the frequencies of genes for a certain behavior can be amplified and affect the behavior of the entire population without changing its overall genetic constitution. Unlike economic theory where the feedback mechanism of the multiplier effect can be modelled mathematically and result in quantitative predictions which can be verified, in sociobiology no attempt has been made to quantify the effect. Instead it is used to explain in an ad hoc fashion phenomena in which small variations in genotype result in an unexpectedly large effect on behavior (p. 11). In order to rationalize the opposite situation in which the multiplier effect fails to operate, a threshold level is postulated, whereby a certain unspecified degree of behavioral complexity is required before the multiplier effect appears. Coupled with the various forms of selection outlined above, the multiplier effect and threshold level make it possible to explain the evolution of the behavior of any society.

The introduction of these ad hoc modes of explanation is a graphic illustration of one aspect of Kuhn's theory of normal science.[10] Once a paradigm for a discipline has been established, problems in that field become puzzles that must have a solution within the context of the paradigmatic theory. If a set of phen-

omena arise that are in seeming conflict with the theory, explanations can always be invented to reconcile the theory with the observations. It is noteworthy that such a new science as sociobiology already shows these characteristics of an aging theory requiring extensive patchwork.

IV

Given its severe methodological flaws, the theory of sociobiology cannot rely on the traditional scientific tests of validity in order to achieve acceptance and legitimacy. At the beginning of this paper we referred to the extensive publicizing of sociobiology in the media which have served as a massive advertising campaign for "The New Synthesis." Likewise Wilson's appearance at symposia in psychology, anthropology and sociology are indications that he wishes to enlist the support of social scientists for his program. These social sciences are becoming increasingly influential in determining which theories of human behavior become accepted, and more importantly, in determining how these theories are applied in the formation of social policy.

We now address the nonrational methods Wilson employs in his book itself.[11] Wilson is well aware of the bad reputation of previous biological determinists whose science was intimately connected to their politics. These biological determinists include the Social Darwinists, Konrad Lorenz, such modern advocates as Ardrey and Morris, and finally scientists like William Schockley. Wilson would like to remove all suspicion of ideology and politics from his work. His emphasis on the extreme reductionist program, on the structure of scientific theories, on the scientific method, and on the various fallacies that plague the unwary scientist all serve to put the reader on notice that he or she is dealing with a scientist whose theory is deserving of the greatest respect.

In addition, Wilson makes use of the technique of reliance on authority. Since his is a scientific text, authority does not mean reference to the great thinkers of the past, but rather to well established scientific theories that can be used as fundamental theories for sociobiology. As we have seen evolutionary population biology serves as a primary fundamental theory. Such a reliance not only serves the methodological purpose of resting sociobiology on a firm foundation, but also serves a subtle psychological function. The fundamental theory of population biology is a rigorous, highly mathematical theory. In fact in the context of the prevailing popular views about science, it is almost a requirement that a scientific theory be expressible in mathematical form in order for it to be considered as a body of rigorous reliable knowledge. A mathematical theory can, however, be very forbidding especially for the lay reader or even for the biologist who has not been trained in the quantitative areas of the field. Consequently the presentation of a 40 page discussion of the principles of population biology containing approximately 50 equations comprehensible only to a reader trained in higher mathematics, tends to deter the nonexpert reader from criticizing the remainder of the work. Although Wilson does allude to the tentative nature of sociobiological theorizing, his method of presentation suggests that the entire book has the same authoritativeness as the chapter on the fundamental science.

One might suppose from the title of Chapter 4, "The Relevant Principles of Population Biology," that everything in the chapter is supposed to be important for the understanding of the remainder of the book. In fact, essentially none of the chapter's elaborate mathematical apparatus is needed for an understanding of the material in the succeeding chapters. Except in a very few instances, the concepts of sociobiology are developed in a primarily qualitative fashion. The mathematics seems to have been introduced for the sole purpose of imputing to sociobiology the rigor and exactness of the fundamental theory of population biology.

Moreover, it is not clear that these "Relevant Principles of Population Biology" are even relevant. In a section of Chapter 4 entitled "Polygenes and Linkage Disequilibrium" (p. 70), Wilson discusses a difficulty with the models of classical population genetics. He points out that the environmental processes which direct the course of natural selection and evolution are directed not at individual genes but at the entire organism. Only if a trait subject to the selection process is well-defined and is controlled by a single gene or by several genes that act independently of each other (in technical language, polygenes whose loci are not linked) is the classical theory valid. As we mentioned above, many of the traits considered by sociobiologists are reifications, having no fundamental biological significance. These traits are almost certainly not controlled by a single gene or even by a few independently acting genes.

Furthermore, Wilson himself cites the work of Franklin and Lewontin[12] who have shown that even for well-defined traits, if interactions between genes are common, then the methods of population genetics applicable to the analysis of independently acting genes is invalid and the entire chromosome (which contains a multitude of genes) must be treated as a unit. One would guess that in view of the complexity of the social traits that sociobiologists are trying to explain, these traits, even if they are biological, would be highly subject to the effects of these interactions. Since nearly all of the methods and principles discussed by Wilson in Chapter 4 depend on the assumption of single genes or at worst several noninteracting genes, these methods and principles are probably totally irrelevant to the study of sociobiology. It is precisely because the fundamental theory of population biology is so far removed from sociobiology that so little of the fundamental theory is actually applied to sociobiology in the remainder of the book.

Wilson closes the section on "Polygenes" by stating that the simpler classical theory "provides an entrée into the first analyses of group selection." "The adjustment of one-locus theory, on which this and most branches of theoretical

population genetics still rest, to the new locus-interacting theory is a task for the future. It remains for sociobiologists to exploit both levels as opportunity arises" (p. 70). Notwithstanding these qualifications, Wilson gives no concrete evidence that sociobiological theory which involves complicated and as yet genetically unanalyzed traits can be founded on the principles of population biology. Chapter 4 is for show.

V

In view of the lack of scientific content in the theory and the propagandistic devices used to legitimate it, sociobiology seems to be best categorized as an ideology, i.e., a mode of explanation designed to rationalize some existing set of beliefs or conditions. As has been pointed out, biological determinist theories such as sociobiology postulate the existence of human universals that are assumed to be genetically controlled. It is a small step, though not a logically necessary one, to the view that these universals are unchangeable. Consequently biological determinist theories have usually tended to justify the status quo, since it is assumed that the current economic and political structure has evolved in a natural way in order to reflect the requirements of biologically determined human behavior. Attempts to change society by, for example, eliminating inequalities are chimerical, since these inequalities are the result of biological differences in people. This mode of explanation is of course totally ahistorical; it ignores the fact that social and political institutions are creations of people, and that these institutions were set up at least in part to support the interest of particular groups within the society.

Jürgen Habermas,[13] among others, has discussed the progression of ideologies used to legitimate societies. Tradition (including religions) were the primary source of legitimation in pre-capitalist societies. At the dawn of capitalism, Adam Smith's laws of economics with its concomitant philosophy of laissez-faire

justified the era of individualism. Today, in light of pervasive government influence on all aspects of society, decisions are justified by showing that they are in accordance with principles of instrumental reason as exemplified by scientific findings. A theory such as sociobiology, which claims scientific objectivity and validity for itself, can thus become a means of legitimizing decisions made in the society. Perhaps the most important ideological use of sociobiology to date is the justification of sexism.

For various political, economic, and social reasons which are beyond the scope of this paper, the struggle of women to overcome inequalities and to secure an equal place with men in our society has met with increasing resistance in the late 1970's. The loss of momentum in the effort to pass the Equal Rights Amendment, various Supreme Court decisions involving pregnancy benefits and abortions, and the increasing disparity in wages between men and women are only some of the manifestations of this trend. The timing of the appearance of sociobiological theories with its rationalization of inequalities between the sexes can only lend support to the hypothesis that scientific theories, especially those which impinge on the study of the human species, are a product of their times.

The sexism in sociobiology appears on many different levels. The language used in the propounding of the theory is so sexist that it is often difficult to determine whether the word "man" is meant to refer to human males or to any member of the human species. This distinction is important in sociobiology since several of the fundamental traits such as aggression and entrepreneurship are assumed to be not universal traits but rather traits characteristic of males only. The sexism inherent in the characterization of these traits as male is extended by stating that male dominance over female is a human universal. Thus an ideological component is built into the fundamental assumptions of the theory itself.

Furthermore the major mechanism of evolution is assumed by the sociobiologists to be sexual selection in which the males compete among themselves and the females choose the winners. This mechanism of sexual selection is only one of a number of possible mechanisms of natural selection and it is only an assumption that it is the important form of selection in human evolution. According to the process of sexual selection as described by sociobiologists, the traits that are being selected for are primarily male ones, and only the passive trait of being able to pick a winner is left for the females. From such assumptions, Wilson's conclusion, presented in The New York Times Sunday magazine for mass readership, is inescapable: "Thus even with identical education and equal access to all professions, men are likely to play a disproportionate role in political life, business, and science."[14]

The ideological component of sociobiology is here explicit. By taking advantage of its scientific status, acquired as we have seen by questionable means, the theory is able to rationalize aspects of society that perpetuate the domination of some groups over others. It is especially disturbing to us as academics that some social scientists are adopting sociobiological theory in their own disciplines in an effort to bolster their disciplines' reputations as rigorous sciences. It is our hope that detailed analyses of sociobiology from the point of view of these "soft" sciences will highlight the intellectual sterility of the reductionist program, and will encourage social scientists to take a more critical view of the ideological underpinnings of the "harder" sciences they have sought to emulate.

Acknowledgements: We would like to thank E. H. Egelman, S. J. Gould and R. C. Lewontin for their many constructive criticisms and comments.

Notes

1. E. O. Wilson, Sociobiology: The New Synthesis, (Harvard University Press, Cambridge, 1975). All page references in the paper refer to this book.
2. B. Rensberger, "Sociobiology: Updating Darwin on Behavior," New York Times, May 28 1975, p. 1.
3. A detailed discussion and list of references can be found in: J. Alper, J. Beckwith, and L. Miller, "Sociobiology is a Political Issue," in The Sociobiology Controversy, ed. A. Caplan, (Harper and Row, N.Y., 1978), scheduled publication date.
4. See for example, "Sociobiology: A New Biological Determinism," in Ann Arbor Science for the People, Biology as a Social Weapon, (Burgess, Minneapolis, 1977), and M. Sahlins, The Use and Abuse of Biology, (University of Michigan Press, Ann Arbor, 1976).
5. J. Schwinger, as quoted by E. O. Wilson, p. 27.
6. A few comments on reductionism may be in order here since the term has been used with various denotations and connotations. By reductionism we mean the attempt to show that for a set of statements describing a set of phenomena (or for the parameters used in such a description), there exists an equivalent or synonomous set of statements derived from a theory. We intend no derogatory connotations in this characterization. There are, however, two possible difficulties in the reductionist program. First, the theory may not be rich enough to describe the phenomena, and second, there may be problems inherent in the procedure of equating one level of description to the other. Most philosophically interesting problems concern themselves with the second type of difficulty. Our objections to sociobiology arise from the first.

7. E. O. Wilson, Bioscience, 26, 187 (1976).

8. T. Dobzhansky, Current Anthropology, 4, 138 (1963).

9. E. O. Wilson, New Scientist, 13 May 1976, p. 342.

10. T. S. Kuhn, The Structure of Scientific Revolutions, 2nd Edition, (University of Chicago Press, Chicago, 1970).

11. The importance of the use of nonrational methods in the justification of scientific theories has been discussed by P. Feyerabend in Against Method, (New Left Books, London, 1975).

12. I. Franklin and R. C. Lewontin, Genetics, 65, 707 (1970).

13. J. Habermas, Toward a Rational Society, (Beacon Press, Boston, 1970), p. 97ff.

14. E. O. Wilson, New York Times Magazine, 12 October 1975.

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SOCIOBIOLOGY IS A POLITICAL ISSUE

by

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Sociobiology Is a Political Issue

Joseph Alper, Jon Beckwith, and Lawrence G. Miller

In this article we wish to expand our discussion of certain issues brought up briefly in previous critiques. The question of the political implications of sociobiology has become increasingly controversial. We believe it is important to point out the political content of the theory. We also seek to show how in concrete terms, sociobiological ideas in their most politicized form are being disseminated. The political content of sociobiology is especially evident in its treatment of sex roles. We will, therefore, discuss the central role of sexism in the formulation of sociobiological theories and in its social impact.

SOCIOBIOLOGY GOES PUBLIC

Sociobiology has been proclaimed as a scientific theory of the dynamics of human and animal societies. The publicity which surrounded the appearance of E. O. Wilson's *Sociobiology: The New Synthesis* emphasized the supposed importance of this new field for social planning. "In charting the future course of society, Wilson believes man will need all the help he can get from the new field of sociobiology."[1] Robert Trivers, one of the leading theorists of sociobiology, has stated:

The authors are particularly grateful to Freda Salzman and Marian Lowe for pointing out the importance of the sexist assumptions and structure of sociobiology and for helping us develop the analysis presented here. In addition, we thank Barbara Beckwith, Tedd Judd, Bob Lange, Judy Livingstone and Del Walicke for their detailed critique of the curriculum, *Exploring Human Nature*.

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> It's time we started viewing ourselves as having biological, genetic, and natural components to our behavior, and that we start setting up a physical and social world to match those tendencies.[2]

What kind of world do sociobiologists see as matching "those tendencies?" Since, according to sociobiology, the tendencies to sex-role division of labor, competition and hierarchies are, to some extent, genetically controlled, there are limitations on how much these aspects of our behavior can be changed. While Wilson has pointed out the dangers of the "naturalistic fallacy of ethics which uncritically concludes that what is, should be,"[3] he has still consistently offered us advice on *what can be* derived from sociobiological wisdom.

How we structure our societies, how we deal with inequalities and other fundamental social questions, ideally depends on ethical principles. But what is the origin of these ethical principles? Sociobiology, according to Wilson, suggests an "evolutionary approach to ethics."[4] Perhaps ethics should be "temporarily removed from the hands of the philosophers and biologicized." In developing these principles, we must be aware that "while few would disagree that justice is an ideal state for disembodied spirits," "the human genotype and the ecosystem in which it evolved were fashioned out of extreme unfairness."[5] In other words, no matter how much we might wish for a fair and egalitarian society, we must recognize the limitations on how far we can move in reaching these goals. The limitations derive from an understanding of our evolutionary history. For instance, as we discuss in detail below, Wilson suggests that sociobiological theory leads to the prediction that women are unlikely to occupy certain of the high-status positions in society.

In summing up his work on the last page of his book, Wilson foresees constraints on possible social changes:

> If the planned society were to deliberately steer its members past those stresses and conflicts that once gave the destructive phenotypes (aggression, dominance, violence) their Darwinian edge, the other phenotypes (cooperativeness, creativity) might dwindle with them.[6]

This is rather a new twist in biological determinist theorizing. It does not say that we cannot change unwanted aspects of our behavior, but rather that if we change them too much, we may lose other more desirable qualities.

Perhaps if such statements were limited to academic journals and academic discussions, they would not be of much concern to us. However, no matter how academic the intent of the proponents, theories which deal with human behavior and human social arrangements have often had significant social impact. This has been particularly true of genetic theories of human behavior from Social Darwinism and eugenics to the recent studies on genetics and intelligence and on criminality.[7] In the case of sociobiology it is not merely that these ideas will eventually leave the university and become widely known because of their own intrinsic appeal; but rather that sociobiologists themselves have been extremely active in ensuring that their theories and the implications of the theories receive the widest public attention.

E. O. Wilson and Harvard University Press launched a massive public relations campaign rare for a technical book in the natural sciences. Even before its publication, a front page article appeared in the *New York Times,* announcing the book and its "revolutionary" implications for human societies.[8] Subsequently, three full-page ads were taken in the *New York Times Book Review* section. Wilson was extensively interviewed, on TV and in the press, and stories appeared in *People* magazine,[9] *House and Garden* magazine,[10] *The National Observer*[11] and the Boston *Globe,*[12] with such titles as "Sociobiology is a New Science with New Ideas on Why We Sometimes Behave Like Cavemen" and "Sociobiology: New Theory on Man's Motivation." This attempt to reach the public with sociobiological theories has continued since the publication of Wilson's book with recent articles in the magazines *Mother Jones*[13] and *Psychology Today* (an interview with Irven DeVore).[14]

In addition, sociobiologists have been proclaiming the importance of their field at a wide range of professional meetings. At the 1976 annual convention of the American Group Psychotherapists' Association, they stated that "clinicians and students of human groups will be able increasingly to base their work on foundations rooted in zoology and primatology." Sociobiologists organized several major sessions at the 1976 meetings of the American Anthropological Association, and Wilson gave major addresses at the eastern meetings of the American Psychological Association and the American Sociological Association in 1977.

One of the areas where, in the past, biological determinist theories

have had their most immediate impact is education. This has been no less true for sociobiology. College and university sociobiology courses have proliferated. In an article in *The American Biology Teacher* (a magazine for high school biology teachers), Paul Gastonguay called for the teaching of sociobiology in high schools, arguing that "several crucial dilemmas faced by our present day society can be quantifiably related to analogous phenomena in other animal species."[15] There already exists a high school curriculum, *Exploring Human Nature,*[16] prepared by Harvard sociobiologists Irven DeVore and Robert Trivers. This curriculum, which is used in over one hundred school systems in twenty-six states,[17] essentially presents sociobiological theory as fact, assuming without question that human behaviors can be explained according to evolutionary principles. Then the students are asked to look at the world around them and explain various social behaviors in terms of their adaptive significance. For instance, the following questions are posed in that way. "What in our evolutionary past led to the human family?" "Why don't females compete? Why aren't males choosy?" "How did the pair bond become part of human nature?" The curriculum is constructed so as to play down the role of cultural factors and cultural evolution in such behaviors. A section entitled "How are Behaviors Inherited?" includes the following statement:

> But some men, because of their genetic construction, will make better soldiers and will probably be happier in their profession than many others. Fortunately modern society offers many different occupations, and differently endowed persons are likely to find one that suits their dispositions.

The obvious popular appeal of sociobiology, created in part by the extensive proselytizing efforts of Wilson and his colleagues, has led to a second wave of sociobiology books and films. In the last year, at least two new, more popular books have appeared: R. Dawkins's *The Selfish Gene* (with a foreword by R. Trivers),[18] and D. P. Barash's *Sociobiology and Behavior* (with a foreword by E. O. Wilson).[19] An illustration of how sociobiological thinking is extended in these books appears in the following quotation from Dawkins's book:

> [Our genes] swarm in huge colonies, safe inside gigantic lumbering robots, sealed off from the outside world, communicating with it by tortuous indirect routes . . . they created us body and mind; and their preservation is the ultimate rationale for our existence.[20]

In the foreword to Barash's book, Wilson warns us that sociobiology will "help us learn what we really are and not just what we hope we are." According to Barash, sociobiology helps us to understand "why women have almost universally found themselves relegated to the nursery, while men derive the greatest satisfaction from their jobs."[21]

In these books, sociobiology is taken to its ridiculous extremes. For instance, Barash uses the principles of this new science to explain a variety of aspects of our social behavior.

> Sociobiological theory would predict that adults with the most to gain and the least to lose would be the most eager adopters, and certainly this is true in the United States where childless couples are the predominant adopters.[22]

Do we really need sociobiology to explain this societal phenomenon?

Possibly the most blatant example of the social implications of sociobiology and a possible harbinger of things to come is a new film entitled *Sociobiology: Doing What Comes Naturally.*[23] The film, which is based mainly on interviews with Wilson, DeVore and Trivers, is designed for high school and college students. It lays heavy emphasis on sex role differences, arguing against the "assertions of the women's liberation movement."[24] While war is not condoned, Trivers, in this film, says that a major motivation for war is the innate need for men to proliferate their genes by impregnating the women of the defeated enemy. Social class differences are implied to be genetically programmed. Ant societies are used as a model: "Once a worker, always a worker. There is no upward mobility here."[25] While the film is clearly a vulgarization of sociobiological ideas (a woman in hot pants is used to symbolize women's natural role as sex objects), its claims and its symbols are a logical extension of the statements by sociobiologists in the film. The film openly presents a justification for a sexist and class society.

THE ASSUMPTIONS OF SOCIOBIOLOGY

The kind of society for which sociobiology provides a rationale is determined by the very assumptions upon which this new "science" is based. Sociobiologists, like other scientists, make critical assumptions when formulating their theories. Some of these assumptions are well-grounded, supported by data from other fields: for example, the existence

of genes and the predictable effects of some kinds of evolutionary pressure upon animal populations. Others, especially those concerning humans, are simply speculative. For instance, the view that contemporary "primitive" societies are similar to societies which preceded our own has been extensively criticized.[26] In our earlier critiques we have analyzed in detail other fundamental assumptions of the theory.[27] We have shown that the hypotheses that *human* social behavior is subject to evolutionary pressures, and that a genetically based human nature exists, are both open to serious questions. Thus, evolutionary arguments founded on animal data cannot be directly applied to the complex interactions both within and between human societies; and while humans are animals and have certain obvious genetic traits, the extent to which genes influence complex social behavior is essentially unknowable at present.

These assumptions form the basis of sociobiological theory. Thus, human behavior can be explained in biological terms, and biological solutions can be suggested for human problems. But since a biological basis for human behavior cannot be determined "scientifically," the political and social biases of the sociobiologists must play a large role in the formulation of their theory of human nature. Thus, despite the sociobiologists' supposed disregard for social, historical and political factors which shape human action, these factors are incorporated into the theory in the form of the behavioral traits suggested as fundamental to human society. The view of a society characterized by such traits as "indoctrinability," "spite," "male dominance over women," "deception and hypocrisy," and "xenophobia," ultimately reflects a particular social, political and historical analysis. From this analysis flow specific political consequences.

THE SEXISM IN SOCIOBIOLOGY

The inherent political and social implications of sociobiology can perhaps be best illustrated by an investigation of the treatment of sex roles. D. P. Barash, in his book on sociobiology, emphasizes the importance of sex differences in the theory: "Sociobiology relies heavily upon the biology of male-female differences and upon the adaptive behavioral differences that have evolved accordingly. Ironically, mother nature appears to be a sexist."[28] These statements are of course accompanied

by the usual hedges; e.g., "sexism is recognition of male-female *differences;* however it does not imply either sex is *better;*"[29] and the biological underpinnings of the theory may or may not apply to humans. Wilson himself, in discussing male-female division of labor, maintains that "even with identical education and equal access to all professions, men are more likely to play a disproportionate role in political life, business, and science," and "my own guess is that the genetic bias is intense enough to cause a substantial division of labor even in the most free and most egalitarian of future societies."[30] It is our contention that this sexism (using the word as ordinary people do) in sociobiology is so embedded in the theory that it can not be simply excised. Furthermore, such disclaimers as Barash's that "evaluation of male-female differences in human behavior should not be construed as supporting its propriety"[31] or Wilson's pious hopes that his guesses even if correct "could not be used to argue for anything less than sex-blind admission and free personal choice"[32] ignore the realities of our society which has discriminated against women and minority peoples for so many years.

The sexism in sociobiology is an outgrowth of the theory itself. The sociobiological theory of human behavior begins by attempting to identify those traits that are common to people in all cultures, assuming of course, that such universal traits exist.[33] The universality of a trait is then deemed to be an argument for the biological basis of that trait.[34] In addition to searching for universals, Wilson maintains (again with the appropriate hedges) that conservative traits, those that remain constant throughout the order Primates, are "the ones most likely to have persisted in relatively unaltered form into the evolution of *Homo.*"[35] The social traits that are both universal and conservative serve as the basis for sociobiological hypothesis formation. These traits, listed in Table 27.1 of *Sociobiology,* are "aggressive dominance systems with males dominant over female; scaling of responses, especially in aggressive interaction; prolonged maternal care; pronounced socialization of young; and matrilinear organization."[36] Since it is argued that males are naturally more aggressive than females, we see that all of the important traits listed by Wilson are based on sex differences. Furthermore, many of the other social traits that are taken to be universal can be derived from those more fundamental traits. Territoriality, xenophobia, and waging war, cited as universals by sociobiologists, are all related to

aggression. Or in the words of Fox, as quoted by Wilson, the dominant male in hominid societies was likely to be "controlled, cunning, cooperative, attractive to the ladies, good with the children, relaxed, tough, eloquent, skillful, knowledgeable and proficient in self-defense and hunting."[37] The implicit and explicit sexism in this list needs no comment.

The arguments given above for the biological nature of sex-related behaviorial traits focus on the universality of these traits in all human cultures and on the appearance of these traits in other primates. Attention is also drawn to the relation between these traits and differences in sex hormone levels in males and females and to the fact that these traits manifest themselves at an early age before differential socialization could occur. Probably the most important work discussing the biological basis of sex differences is *The Psychology of Sex Differences,* by E. Maccoby and C. Jacklin,[38] which is cited by Wilson in his *New York Times* article. A detailed critique of the evidence in support of the thesis that men are naturally more aggressive than women is given by Chasin.[39] Chasin points out that the evidence on nonhuman primates is selected to confirm the biological determinists' hypotheses. Different species of primates differ dramatically in their aggressiveness, and even groups within the same species differ depending on the habitat. Thus, baboons living on the plains (one of the sociobiologists' favorite species) show the classic patterns of male dominance, while forest baboons of the same species show little aggression and no male dominance hierarchies. The cross-cultural evidence cited by Maccoby and Jacklin is similarly circumscribed. Chasin discusses the Tasaday people of the Philippines, Pygmies, the Chinese, and the !Kung bushmen before they were resettled as examples of people living in societies in which boys are not more aggressive than girls and in which the sexual division of labor is much less pronounced then is the case in our own society.

The hormone research indicating that higher levels of male hormones result in more aggressive behavior is inconclusive at best. For example, it is conceivable that increases in male hormone levels are the result of rather than the cause of the aggressive behavior. Finally, the arguments that aggressive behavior appears before socialization can occur are difficult to accept in view of the evidence showing that babies of different sexes are treated differently from birth.[40] The possibility that parents' reactions to their child's behavior is influenced by their precon-

ceptions of proper and expected male or female behavior is discounted by Maccoby and Jacklin.

The tendency to discuss only the evidence supporting the thesis that sex-related behavior is biologically controlled might be seen as resulting simply from the sexist biases that pervade our society and which may even operate below the level of consciousness. (Indeed, Maccoby and Jacklin maintain that they are feminists!)[41]

But deeper than the sexist biases in the sociobiologists' selection of the universal traits is the sexism that is inherent to the structure of the theory of sociobiology as it exists today. Sociobiology relies almost totally on Darwinian sexual selection as the mechanism for the evolutionary development of characteristics. This theory of sexual selection operates in the following manner. A single male animal can impregnate many females. Thus, in order for a species to produce as many offspring as possible it is necessary for all the females to be reproductively active, but only a small fraction of the males need contribute their sperm. In order for the fittest offspring to be produced, only the fittest males should engage in reproduction. In evolutionary biology an individual's fitness is measured by its success in propagating its genes in the next generation. Fitness is equated with reproductive success and so competition among the males for the available females is inevitable. It is this competition that gives rise to the biological basis for aggression, aggressive dominance systems with male dominant over female, territoriality, and entrepreneurship. Males possessing the most aggressive traits therefore succeed in propagating their genes through successive generations by means of their and their offspring's superior mating ability. Males are more aggressive and cunning than females because the evolutionary pressures of sexual selection force them to be so. This theory of sexual selection, however, is only one of a number of possible forms of natural selection and its importance in the evolution of the human species is an untested hypothesis.

The implications of this theory are striking. The important traits that have been selected for in evolutionary history are male traits. Only males need compete; therefore selection operates on the genetic variability in males. Genetic variability in females is relevant to the evolutionary process only when females choose from among competing males. Females are thus the passive carriers of the selected male genes. Is it a coincidence that the conventional stereotypes of the passive woman

acted upon by aggressive men carries over into the sociobiologists' view of the female role in the evolution of the species?

Sociobiology cannot be divorced from its sexism. Not only are the postulated human universals sexist, but the asserted mode of their propagation in evolution is sexist as well. This pervasiveness of the sexism in sociobiology is camouflaged by the careless and sexist language used by the sociobiologists. An insistence on the use of the word "person" instead of "man" when referring to a member of the human species is often viewed as frivolous nitpicking, but in sociobiology the distinction is crucial. The title of Chapter 27 of Wilson's book is "Man: From Sociobiology to Sociology," but the human universal traits of "man" that are cited by Wilson are really traits of stereotypical males. Aggression and territoriality are universals in males and are not species universals. Once the sexism of the language is exposed and the ambiguous use of the word "man" made clear, it becomes manifest that sociobiology carries with it the implication that human social behavioral traits evolved primarily through selection on male traits. Much of "Man's" evolutionary history is really the history of "men." Female social behavior, which is not readily explainable on the basis of sexual selection, is—like the creation of Eve in Genesis—just an afterthought.

CONCLUSION

Sociobiological theories, then, contain both implicit and explicit political implications and have been promoted in a fashion emphasizing these implications. As we have pointed out elsewhere, theories similar to sociobiology are not new: what have been termed evolutionary positivist theories extend back at least a century, and have included Social Darwinism and some contemporary ethology. The explosion of interest in sociobiology manifested by many publications indicates a revival of interest in these theories. This revival is one example of the increasing reliance upon science to explain a host of social problems and the proposal of scientific solutions to such problems. This reliance has been clearly demonstrated in recent controversies concerning the importance of scientific notions of intelligence (I.Q.) and of biological bases of criminality (XYY chromosome). In those controversies the assumption of the genetic basis of human behavior has been used to rationalize social inequalities and deviant behavior. Problems such as the failure of compensatory

education and the increasing crime rate, both of which are rooted in the structure of society, are studied as scientific problems susceptible to scientific analysis. Given the social context in which these theories were developed, it is not surprising that they were very rapidly presented to the public as scientific truth and have had significant social consequences.

As sociobiologists have been quick to point out, sociobiology promises an extension of this trend. For example, the role of women in society is now a highly debated issue. The struggle over the passage of the Equal Rights Amendment and the recent Supreme Court decision denying pregnancy benefits to workers (General Electric Co. *v.* Gilbert, 1976) are only two of its manifestations. Sociobiological arguments concerning innate sex-role differences clearly have relevance to the debates on these issues. Since these arguments are presented as objective science, they can be used to provide "rational" solutions to essentially political problems. Considering the history of the use of biological determinist ideas, there is good reason to fear that the development of sociobiological theories in the present social context will have pernicious social consequences.[42]

NOTES

1. N. McCain, "Sociobiology: New Theory on Man's Motivation," Boston *Sunday Globe,* November 16, 1975.
2. From sound track of *Sociobiology: Doing What Comes Naturally.* A film distributed by Document Associates, Inc., 880 Third Avenue, New York, N.Y.
3. E. O. Wilson, "Human Decency is Animal," *The New York Times Magazine,* October 12, 1975.
4. E. O. Wilson, *Sociobiology: The New Synthesis* (Cambridge, Mass.: Harvard University Press, 1975), p. 564.
5. *Ibid.,* p. 562.
6. *Ibid.,* p. 575.
7. Ann Arbor Science for the People, *Biology as a Social Weapon* (Minneapolis: Burgess, 1977).
8. B. Rensberger, "Sociobiology: Updating Darwin on Behavior," *New York Times,* May 28, 1975, p. 1.
9. "Sociobiology is a New Science with New Ideas on Why We Sometimes Behave Like Cavemen." Interview in *People* magazine, November, 1975.

10. "Getting Back to Nature—Our Hope for the Future," Interview in *House and Garden,* February, 1976, p. 65.
11. P. Young, "Is Behavior Inherited?" *The National Observer,* August 16, 1975, p. 1.
12. N. McCain, *op. cit.*
13. R. Brown, "The Monkeys Who Kill their Young," *Mother Jones,* January, 1977, p. 32.
14. S. Morris, "The New Science of Genetic Self-Interest," *Psychology Today* 10, no. 9 (1977), p. 42.
15. P. R. Gastonguay, "A Sociobiology of Man," *The American Biology Teacher,* November 1975, p. 481.
16. Education Development Center, *Exploring Human Nature* (Cambridge, Mass.: EDC, 1973).
17. Information obtained from Educational Development Center.
18. R. Dawkins, *The Selfish Gene* (New York: Oxford University Press, 1976).
19. D. P. Barash, *Sociobiology and Behavior* (New York: Elsevier, 1977).
20. Dawkins, *op. cit.,* p. 21.
21. Barash, *op. cit.,* p. xv.
22. *Ibid.,* p. 313.
23. *Sociobiology: Doing What Comes Naturally.* A film distributed by Document Associates, Inc., 880 Third Avenue, New York, N.Y.
24. *Ibid.,* from the brochure advertising the film.
25. *Ibid.,* from the sound track.
26. See, for instance, M. H. Fried, *The Evolution of Political Society* (New York: Random House, 1967), especially pp. 170–174.
27. See other articles in this volume and "Sociobiology: A New Biological Determinism" (an expanded version of the *BioSciences* article) in Ann Arbor Science for the People, *op. cit.*
28. Barash, *op. cit.,* p. 283.
29. *Ibid.*
30. Wilson, *The New York Times Magazine, op. cit.*
31. Barash, *op. cit.,* p. 283.
32. Wilson, *The New York Times Magazine, op. cit.*
33. See M. Sahlins, *The Use and Abuse of Biology* (Ann Arbor: University of Michigan Press, 1976) for evidence that important human social behavioral traits are not universal.
34. For a discussion of the fallacy in this argument, see R. C. Lewontin, "The Analysis of Variation and the Analysis of Causes," in N. Block and G. Dworkin, *The IQ Controversy* (New York: Pantheon, 1976), p. 179.
35. Wilson, *Sociobiology: The New Synthesis,* p. 551.
36. *Ibid.,* p. 552.
37. *Ibid.,* p. 569.
38. E. Maccoby and C. Jacklin, *The Psychology of Sex Differences* (Stanford: Stanford University Press, 1974).

39. B. Chasin, "Sociobiology: A Sexist Synthesis," *Science for the People Magazine* 9, no. 3 (1977), p. 27.
40. S. Goldberg and M. Lewis, "Play Behavior in the Year Old Infant: Early Sex Differences," *Child Development* 40, no. 21 (1969).
41. Maccoby and Jacklin, *op. cit.*, p. 12.
42. Recent examples of the overt political use of sociobiology include the August 1, 1977, cover story in *Time* magazine and an article in April 10, 1978, issue of *Business Week* entitled "A Genetic Defense of the Free Market."

SELECTED BIBLIOGRAPHY

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THE ETHICAL AND SOCIAL IMPLICATIONS OF SOCIOBIOLOGY

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In the light of some currently prevailing views about the nature of scientific theories, the title of this paper is a contradiction in terms. Sociobiology, according to its practitioners, is a science like chemistry and physics, and as such it can be characterized by the same tenets applicable to all sciences. A scientific theory is said to be objective; its content is independent of the investigator and of the particular environment in which he or she works and lives. The scientific method by which the theory is produced is also objective; the logical argumentation and the evidence presented are either valid or invalid sub specie aeternitatis. Consequently, a scientific theory is value free. It entails no ethical or political implications since values have no objective reality but are rooted in human culture.

The naivite of these views, though not widely recognized by scientists, is becoming increasingly well known so that just a few observations are necessary here. The form of scientific inquiry is determined to a great extent by the dominant forces of society (Beckwith and Miller, 1976). The problems studied and the direction research takes to solve these problems are controlled to a greater or lesser degree by the funding of projects by the government and by large corporations (Noble, 1977). Furthermore, very often scientific and technical research is of a type that can be used either directly or indirectly for the purpose of exerting economic or social contgol over great numbers of people (Braverman, 1974). The standards for accepting the logic of and the evidence for a new theory reflect not some absolute standard but rather the willingness of scientists to accept a new theory, which in turn depends on the political and social as well as the intellectual conditions of the time (Kuhn, 1970; Feyerabend, 1975).

Sociobiology, a recent offshoot of biology, which attempts to discover the biological basis of the social behavior of animals and humans, is a theory which not only illustrates graphically the lack of objectivity of science but which also contains very serious social and political implications. The major con-

clusion of sociobiology seems to be that the social behavior of people including such traits as sex roles, aggression, territoriality, and altruism are largely determined by our genes and by evolution. If these characteristics of our society are biologically determined, then the present structure of society is natural, thus justifying the status quo. It follows that not only is there little point, but there may be possible grave dangers in trying to overcome sexism and aggression or in trying to change the present economic and social structure since the present society is based on natural selection and other biological principles.

As sociobiologists themselves have pointed out, there is no strict logical deductive chain from the hypothesis that traits are biologically determined to the conclusion that these traits are unchangeable. A trait is said to be biologically determined if it is one which remains relatively constant over all known environments. However, there always exists the possibility that new environments could be provided in which these traits would be modified. This limitation on the causal chain is, however, quite subtle. Very few nongeneticists appreciate the distinction between a biologically determined and an immutable trait. In our society, for all practical purposes the distinction does not exist. For this reason it is particularly alarming that sociobiological theory, which has no proof that the social behavioral traits it discusses are biologically determined, is taught in high school curricula. The Educational Development Center's text Exploring Human Nature (1973) presents sociobiological reasoning in such a way as to inculate in its readers a belief in the inevitability of the status quo.

Sociobiology has its origins in the study of the social behavior of animals. The complex social behavior of bees, ants, lions, and monkeys is familiar to us all, and sociobiology attempts to account for this behavior in terms of biological principles. This field gained widespread recognition with the publication of

E. O. Wilson's text Sociobiology (1975a). This book, whose subtitle is "The New Synthesis," seeks to determine general principles and then apply them to all social animals including humans. Since human beings form a species of animals they follow the same biological laws. Consequently human social and behavioral traits can be seen as universals (common to all people) and these traits can be explained in terms of the same biological principles that apply to other animal species. Wilson states that "[t]he principal goal of a general theory of sociobiology should be an ability to predict features of social organization from a knowledge of these population parameters combined with information on the behavioral traits imposed by the genetic constitution of the species" (p. 5). The "population parameters" referred to are demography and the genetic structure of populations. This program seems to disregard culture, removes philosophical questions, and seeks to subsume such disciplines as anthropology, economics, and sociology in the "New Synthesis under biology.

Sociobiological theory as applied to the human species operates in the following manner. It is postulated that differences in human behavioral traits from one person to another are caused by genetic differences between the individuals. If one then assumes that these traits are adaptive, that is, ensure greater reproductive success to their bearers, then these biologically determined traits can be passed on from one generation to the next. In the course of time, those traits providing the greatest adaptive value will dominate and become universal biologically determined traits. Since many of the traits involved are social ones that do not benefit the single individual, it is further postulated that a trait will also prevail if the individual's relatives, who carry some of the same genes, are benefited by that trait (kin selection).

One of the examples Wilson provides will illustrate the operation of the theory. Homosexuality can be explained using sociobiological concepts. It is assumed that homosexuality is genetic in origin. If it is also assumed that

homosexual members of primitive societies functioned very efficiently in assisting close relatives (some of whom also carried the "homosexuality genes"), then these relatives might have become more reproductively fit, that is, have had more children, and thus the trait of homosexuality would be preserved and propagated. Needless to say, there is no evidence for any of these assumptions.

This example of the explanation of homosexuality is indicative of the untestable nature of sociobiology. The assumptions on which this explanation is based are unprovable since they rely on guesses about early human societies. Even if the assumptions were provable, the possibility of alternative explanations is not ruled out. Why couldn't homosexuality be caused by environmental factors arising in the course of an individual's development? Wilson himself in a section of his book on scientific methodology refers to this error of not ruling out alternative explanations as the well known fallacy of "Affirming the Consequent" (p. 29). Moreover, by using the mechanisms of individual and kin selection together with that of reciprocal altruism (one person helps an unrelated person in the expectation that the receiver of help will eventually reciprocate) and spite (behavior that benefits neither of the interacting individuals) any social behavior at all can be explained, that is, shown to be adaptive. Unlike a science such as astronomy in which speculation also runs wild but in which most of the theories are eventually found to be false as a result of new observations, for sociobiology no phenomena could arise that sociobiologists would be at a loss to explain in terms of their theory. Sociobiology has more in common with an all-explanatory system like Freudianism than with any of the empirical sciences.

It is useful in beginning a critique of the specific postulates of sociobiology to look at the particular behavioral traits and social conditions the theory attempts to explain. Some of the traits and conditions that Wilson cites include indoctrinability, the nuclear family, male dominance and superiority,

blind faith, genocide, warfare, xenophobia, and finally the unequal distribution of wealth and prestige. In the first place there is much sociological and anthropological evidence showing that many of these behavioral patterns are in fact not human universals (Sahlins, 1976). Second, Wilson never explains why these particular traits were chosen or whether there might be other human social traits of equal or more fundamental importance. Wilson seems to have fixed on them because they are the ones most noticeable in our present,society, but it is by no means certain that sociobiologists from a different culture at a different time might not have focused on an entirely different set of traits having little in common with those our sociobiologists chose to study. It is evident that the traits chosen characterize our particular society and that an "objective" theory that will account for these behavioral patterns would go a long way toward justifying that society.

In choosing human behavioral traits that are to be deemed universal, sociobiologists find it useful to make connections with behavioral traits exhibited by animals. Such a connection would constitute evidence that the analogous traits were genetic and also that they arose in humans as a result of evolutionary natural selection. The methodology used in making the connection, however, gives the appearance of mutual reinforcement. First, animal behavior is described in human terms. Examples abound: The word "slavery" is used to describe certain behavioral patterns in ants; in describing "courtship" behavior in animals such terms as "salesmanship," "sales resistence," "coyness," and "machismo" are employed (chapters 13-16); and such elements of behavior as barter, division of labor, and role playing are attributed to animals. Sociobiologists maintain that such terms when applied to animals are being used in a special technical sense and that they are not meant to imply any relationships to human behavior. However, this distinction is easily forgotten and in fact sociobiologists then

use this animal behavior which they had already characterized in human terms to argue that the corresponding behavior in humans has its roots in that animal behavior and is therefore biological in origin. Once again we should note that these universal traits, which are now attributed to animals, seem to be those types of behavior relevant to the society of the researchers themselves. It is no wonder that ethologists are able to explain competitive and sexist traits in people on the basis of studies of animal behavior; the particular human traits of interest to the ethologists were attributed to the animals by the ethologists themselves.

Once the particular traits to be explained have been singled out (by a highly nonobjective, value-laden procedure, as we have seen), the sociobiologists must then show that these traits are biologically determined, that is differences in these traits are due to genetic and not to environmental differences and that these traits remain unchanged unless there are drastic environmental changes. There is no direct evidence for the genetic basis of human behavioral traits, and in fact it is probably impossible to find evidence either for or against the principle. It is impossible to perform controlled experiments on people since we cannot for obvious moral and practical reasons manipulate the environmental and genetic factors affecting people's lives. Another difficulty lies in the fact that human behavioral traits are not objective realities but are rather constructs created by social theorists. It is not axiomatic that there are genes responsible for the expression of such reified traits. Calling such elements "organs" of behavior (Wilson, p. 22) does not provide them with any greater degree of reality. Once again nonscientific presuppositions of the investigator play an important role in determining the shape of a scientific theory.

In all fairness it should be stated that Wilson does not consider his theory to be a biologically deterministic one, but maintains that environmental forces, including culture could play a large role. However, sociobiology when applied

to humans would be meaningless as a discipline if it were not basically biological. The avowed program of sociobiology to base the study of social behavior on the sciences of population genetics and evolutionary ecology precludes, or at least relegates to a subordinate role, the introduction of cultural factors. Inclusion of these factors would require an independent theory of the development of social structures which is of course absent from present sociobiological theorizing. This absence is probably unavoidable since there could be no "new synthesis" in which human sociobiology is incorporated into animal sociobiology and there could be no withering away of anthropology and sociology unless the same biological principles of animal sociobiology can be applied to the human species without recourse to additional cultural hypotheses.

Sociobiology's major flaw seems to be that it fails to recognize that the human species develops by means of two forms of evolution. Biological evolution ensures survival to the descendants of those animals who were more fit reproductively, in other words, were better adapted to the particular environment in which they were living at the particular time they lived. The traits that are passed to their descendants are those traits that these better adapted animals were born with. Biological evolution can be contrasted with cultural evolution, in which acquired traits of one generation are passed directly to the next generation. This type of evolution is of overwhelming importance to the human species. It is a much more reasonable hypothesis, with at least as much evidence in favor of it, that human social behavioral traits are not biological universals passed on by Darwinian evolution but rather cultural traits transmitted by cultural evolution. This latter type of evolution is most directly due to the development of language. People are able to react to their environment, to change both it and themselves, and to pass the knowledge of these changes directly to the next generation. The Lamarckian character of cultural evolution enables human beings to adapt very rapidly to changed in the environment, much more rapidly

than those species that must rely on biological evolution, and has been of tremendous survival value for the human species.

Cultural transmission of social behavior does not preclude recognition of a genetic component in all human behavioral traits. But society is not simply the sum of the behavior of individuals, as the sociobiologists seem to assume. It is rather the product of complex interactions among people and the institutions of society that have arisen in the course of history. People created society which in turn forms the political and social environment for their lives. This environment influences their behavior which in turn can modify society. People always retain the potential for overcoming their environment, revolutionizing social and political institutions, and changing their own nature.

In its attempt to explain all of human behavior in biological terms, sociobiology ignores these complex interactions between genetics and the environment, including society, and assumes that human nature is biologically determined and basically unchangeable. This theory, although presenting itself in the form of objective science, is clearly a product of the political and social climate from which it arose. Both its content and the method used to validate it have been structured to fulfill certain preconceptions including the justification of the status quo and the rationalization of policies maintaining sexual, social, economic, and political inequality.

We now shall examine some of the specific implications of sociobiology. We recall that the social and behavioral traits deemed basic by sociobiologists are claimed to be universal, largely biologically controlled, and consequently natural. Several of the traits which are repeatedly stressed, namely competition, entrepreneurship, and aggressive behavior appear to be especially relevant to Western capitalistic societies. Wilson believes that these traits are universal because they arise out of the universal conditions of economic scarcity. "The members of human societies sometimes cooperate closely in insectan fashion, but more

frequently they compete for the limited resources allocated to their role sector. The best and most entrepreneurial of the role-actors usually gain a disproportionate share of the rewards, while the least successful are displaced to other, less desirable positions" (p. 554).

The use of the derogatory word "insectan" to characterize societies where cooperation rather than competition is the norm is probably the most telling betrayal of Wilson's supposed scientific neutrality. Moreover, his statement is misleading since scores of societies have been studied which are not characterized by a hierarchy of social positions and an inequality of shares (see, for example, Birket-Smith, 1959; Harris, 1968, pp. 369-391; and other references cited in Sociobiology Study Group, 1977). The view that these behavioral patterns characteristic of capitalist societies are universal over time and in all cultures cannot be substantiated.

Rather than being universals, these traits are quite specific to our culture and as such play a large role in forming the ideology of our society. Ideologies both conceal and reveal the underlying assumptions of the structure of a society and function to legitimate it. It has been pointed out that traditions which at one time served this legitimizing function (for example, religion) lost their efficacy at about the time of the French Revolution and that subsequently, with the rise of positivistic schools of thought, more scientific modes of explanation were required for that purpose (Habermas, 1970, pp. 97ff; Gouldner, 1976, Ch. 2). Sociobiology, which sees itself as the science of social behavior, seems ideally suited to provide a credible foundation for the ideology of capitalism. This role of sociobiology in supporting the ideology of capitalism and thus of the status quo is rather abstract and cannot be explored fully in this paper. We turn instead to those implications that are of more immediate consequence.

Whether for good or for bad our society relies on the opinions of certified scientific experts for making social and political decisions. The testimony of

psychologists that Eastern and Southern Europeans ~~were~~ are innately less intelligent than Western Europeans was instrumental in the passage of the highly restrictive Johnson-Lodge Immigration Act of 1924 (Kamin, 1974). On the other hand, the opinion of psychologists and sociologists that segregated schools are inherently unequal to desegregated ones influenced the Supreme Court in the School Desegregation case (Brown v. Board of Education, 1954, see especially footnote 5). If sociobiology is regarded as a science, its proclamations will be taken very seriously, even more seriously than those of the "softer" sciences of psychology and sociology. With these thoughts in mind, we consider the possible effects of sociobiological theorizing on the problems of sexism and racism.

The issue of the role of women in our society has become increasingly important in recent years. Many women have begun to see themselves as an oppressed group, discriminated against in education, in employment opportunities and advancement, and having been relegated to an inferior social status by the necessity of remaining at home taking care of the house and their children. This raised consciousness has resulted in the creation of women's organizations fighting for equal treatment. On the other hand, because of high levels of unemployment, there is increasing fear of the prospect of a new group of people competing on equal terms for jobs and especially for the relatively few available professional and managerial positions. As a result of these economic conditions, as well as other political and social factors which are beyond the scope of this paper, the women's movement, after a period of some gains, has met with increasing resistence in the late 1970's. The loss of momentum in the effort to pass the Equal Rights Amendment, various Supreme Court decisions involving pregnancy benefits and abortions, and the increasing disparity of wages between men and women are only some of the manifestations of this trend. These reverses have led to an increasing intensity of debate as women have become more insistent in their demands.

Such debate, of course, is not a new phenomenon, and during periods of the waxing of women's movements, as for example, the suffragette movement in this country in the early twentieth century, arguments have always been made that women are naturally (in current language, biologically) , both physically and mentally, the weaker sex and so could not expect to play the same role in society as men. Not surprisingly, various scientific evidence and scientific theories have recently been presented purporting to prove the unsuitability of women for positions of responsibility and authority and to demonstrate that women's place is in the home. Of these theories sociobiology is the most encompassing and sophisticated. The coincidence of the appearance of sociobiological theories rationalizing the retreat from the goals of equality is so striking that even Time Magazine (1977, p. 63) in its generally favorable treatment of sociobiology felt compelled to refer to it.

The importance of sex differences in the theory of sociobiology cannot be overemphasized. In his book, D. P. Barash (1977, p. 283) writes: "Sociobiology relies heavily upon the biology of male-female differences and upon the adaptive behavioral differences that have evolved accordingly. Ironically mother nature appears to be a sexist, at least where nonhuman animals are concerned. There may or may not be similar biological underpinnings of sexism in human societies; we do not know." Wilson (1975b) maintains that "even with identical education and equal access to all professions, men are more likely to play a disproportionate role in political life and science." "My own guess," he continues, "is that the genetic bias is intense enough to cause a substantial division of labor even in the most free and egalitarian of future societies." Both authors stress that they are talking merely about differences and that they are not implying that one sex is better. Wilson adds that even if his guess is correct, it "could not be used to argue for anything less than sex blind admission and free personal choice." Nevertheless, such disclaimers ring false in a society such as ours where dif-

ferences have always entailed prejudices. If women are by nature less fitted for the occupations Wilson mentions, which are among the most prestigious in our society, it is difficult to see how women could not be seen as inferior. The combination of a history of centuries of sexual discrimination together with a scientific theory purporting to explain in biological terms the stereotypes upon which this discrimination rests, can only act to reinforce existing prejudices.

We have discussed elsewhere the crucial role that sexual differentiation plays in all aspects of the theory of sociobiology so that only one phase of the argument will be given here (Alper, Beckwith, and Miller, 1978). The fundamental traits upon which the theory is based and from which nearly all of the other traits can be derived, namely, "aggressive dominance systems with males dominant over females; scaling of responses, especially in aggressive interactions; prolonged maternal care, pronounced socialization of young; and matrilinear organization," (Wilson, 1975a, p. 552) are all sex related. It is the supposed biologically based universal aggressiveness and dominance of males that lead Wilson to his conclusion that women naturally stay at home and men go out to hunt or to perform the modern equivalent. The arguments supporting the assertions of the universality and biologically based sex differentiated nature of these traits depend on analogies to primate behavior, anthropological studies of different cultures, physiological research on differences in sex hormone levels and on brain differentiation, and psychological studies on the development of the sex-stereotypic behavioral differences in children. Much of this research and especially the use of this research by sociobiologists is open to serious question and has been subjected to detailed criticism (Chasin, 1977; Salzman, 1977). At the present time the most charitable position on the sociobiologists' view of women is that it is unproven.

There seems to be general agreement among both the opponents and proponents of sociobiology that sexism or at least the doctrine that the biological differences

between the sexes result in different types of social behavior is an integral part of the theory. The disagreement lies in the implications of the theory and the question of the responsibility of the sociobiologists. Are sociobiologists responsible if despite their statements that their theories describe "what is" and not "what ought to be," sociobiology is used as a scientific theory to justify unequal pay or to deny a woman an executive position in some corporation? Since we do not live in an ideal society where people are treated as equals even though they exhibit differences, the realization that sociobiology is only a speculative theory and that even the evidence on which it relies is uncertain, seems to us to be the decisive factor in answering the question.

The issue of the racist implications of sociobiology is a much more delicate one; we are all much more aware of racism than of sexism. Throughout its history, the United States has been plagued by a high degree of racial violence. Our society has made efforts to overcome racism, and academics in particular are usually on thier best behavior when discussing this issue. We can imagine the uproar if the passage quoted above on representation in the professions distinguished between races rather than sexes. There can be no doubt that no matter what qualifications and caveats accompanied the statement, the author of such a statement would be labeled a virulent racist. In the language of the Supreme Court, race is a suspect classification.

From the time of the publication of Wilson's book, discussions concerning the link if any between sociobiology and racism have generated more heat than light. The early statements of the Sociobiology Study Group of Science for the People (SSG) were interpreted to mean that sociobiology was a racist theory equivalent to Social Darwinism and some people in the group felt the need to state publicly that SSG did not believe that sociobiologists are racists. SSG believes that sociobiology is not racist in the usual sense of the term. However, built into the theory is the biological inevitability of intergroup conflict. Given

the history of our country and present political realities, such a theory can be easily used to support doctrines of discrimination. We believe that sociobiologists cannot divorce themselves from the racial implications of their theory.

A race can be defined as "a breeding population characterized by frequencies of a collection of inherited traits that differ from those of other populations of the same species" (Goldsby, 1977, p. 21). Goldsby notes that we can replace "frequency of a collection of inherited traits" with the more precise term "gene frequencies" (p. 25). It is important to emphasize that the concept of race is not completely well defined since there is a great deal of overlapping of the races as defined and since there is no unambiguous way of dividing a species into groups according to the frequencies in the appearance of specific genes. In fact it is questionable whether the concept of race is a useful one at all.

Modern racism is the doctrine that some races are biologically superior to others. At first sight sociobiology appears to be an anti-racist theory since it places such emphasis on the universality of human traits. However, because sociobiology is based on evolutionary theory, genetic differences among people are crucial. Evolution is possible because some genetic differences give rise to phenotypes which have greater adaptive value than others. Bearers of these phenotypes become proportionally more numerous in succeeding generations. We have discussed this process in terms of individual and kin selection. In order to explain such phenomena as warfare, sociobiologists invoke another form of selection, group selection. Wilson (1975a, p. 8) defines a group as "a set of organisms belonging to the same species that remain together for any period of time while interacting with one another to a much greater degree than with other conspecific organisms. The word 'group' is thus used with the greatest flexibility to designate any aggregation or kind of society or subset of a society." People in a group are characterized by a set of gene frequencies that differ from the gene

frequencies of the people in a neighboring group. In a completely analogous mechanism to that of kin selection, people in one group will tend to cooperate in struggle against those in the neighboring group in order to increase the chances of their genotypes prevailing. Group selection thus explains xenophobia and warfare. In Wilson's words, "[o]utsiders are almost always a source of tension" (p. 286). War is the natural outcome of the struggle of one group against another. "Deus vult was the rallying cry of the First Crusade. God wills it, but the summed Darwinian fitness of the tribe was the ultimate if unrecognized beneficiary" (p. 561).

Although Wilson rarely if ever uses the word "race" in his text, it appears that race as defined by Goldsby is one type of Wilsonian group. It is true that much of the technical biological literature restricts the term group to a subset of a population that usually would be much smaller that what would normally be considered to be a race (Maynard-Smith, 1976). Nevertheless, it is clear from the quotation above referring to the First Crusade and from his definition of a group that Wilson's concept is much broader than the technical one. Once we have recognized that a race is simply a type of group, we see that for sociobiology racial conflict is natural.

Up to this point in the argument there is no indication that there are actual racial differences or that one race is superior to another. However, even at this stage the theory has serious implications. Although no justification has been given for the doctrines of racism, racist behavior and prejudice are seen to be natural biological traits that are possibly inevitable and that may even have adaptive value. Barash (1977, p. 311) has written: "Given the evolution of differences and the fact that such differences have been associated with greater genetic similarity within groups than between groups, it seems plausible that a degree of antagonism, the converse of altruism, would occur when representatives of different groups meet...Clearly this suggestion of a possible evolu-

tionary basis for human racial prejudice is not intended to legitimize it, just to indicate why it may occur." Wilson (1975a, p. 286) after describing manifestations of xenophobia, states: "Efforts are then made to reduce them [enemy groups] to subhuman status, so that they can be treated without conscience. They are the gooks, the wogs, the krauts, the commies—not like us, another subspecies surely, a force remorselessly dedicated to our destruction who must be met with equal ruthlessness if we are to survive...At this level of 'gut feeling,' the mental processes of a human being and of a rhesus monkey may well be neurophysiologically homologous." This quotation is striking in that a key aspect of racism, the treatment of a person of a different race as a subhuman, is presented as a biological universal which can be traced to a similarity in the "wiring" of the brains of people and monkeys.

The genetic mechanism for group conflict as described by Wilson is only a hypothesis which is still the subject of controversy among biologists (Maynard-Smith, 1976). Moreover, irrespective of the merits of any of the parties to the biological debate, it would seem that the obvious social, political, and economic causes of racial conflict and war are vastly more important than genetic factors (Sahlins, 1976).

We believe that there is a deeper connection between sociobiology and racism than just the alleged biological nature of intergroup prejudice . Wilson himself goes further in his book: "[T]here is a need for a discipline of anthropological genetics...Variation in the rules of human cultures, no matter how slight, might provide clues to underlying genetic differences, particularly when it is correlated with variation in behavioral traits known to be heritable" (p. 550). The implications of this statement are frightening. The entire race-IQ issue was built on precisely this attempt to show that the differences in measured IQ between blacks and whites are due to differences in their genes. The history of that controversy has shown that it is probably impossible to measure the heritability

of any complex social behavioral trait because of the problems encountered in heritability studies (Kamin, 1974; and Block and Dworkin, 1976). A by no means exhaustive list of the difficulties include (1) the definition and quantification of a complex trait such as intelligence (what does IQ have to do with intelligence?), (2) the flaws (including fraud) in the identical twin studies designed to measure heritability, (3) the impossibility of measuring the separate genetic and environmental components of a trait in cases where the genetic-environmental interactions are strong, (4) the methodological error of using heritability estimates for one group to estimate heritability for another group, and (5) the error of assuming that heritability within a group can tell us anything about the causes of differences between groups.

Furthermore, as Wilson himself indicates, genetic variation between groups is small. Lewontin (1972) has shown that for blood types and presumably for other traits as well, most of the genetic variation in the human species occurs within groups. Cavalli-Sforza (1974) has pointed out that most of the small intergroup differences are literally superficial, that is, skin deep. Most racial differences are due to the adaptation of different groups to their different physical environments (climate, amount of ultra-violet radiation from the sun). Given the number of important unsolved problems in biology, it seems peculiar to worry about "slight" genetic differences and to postulate unprovable biological explanations for social phenomena which might arise from these differences. Arthur Jensen (1969) was explicit in his discussion of the biological origin of IQ differences. Why fund compensatory educational programs if they are destined to fail?

Once the idea of biologically caused differences in behavior is accepted it is only a small step to a belief in racial stereotypes and then to a judgment of which of the alternative behavioral patterns is superior. Throughout history the dominant group in the society, whether racial, political, economic, social, or religious has defined the differences and decided which characteristics are

to be deemed inferior. In the course of time, these determinations are solidified and are perpetuated by the institutions of the society.

This final step of defining racial differences and determining which characteristics are the superior ones is of course absent from sociobiology. But sociobiological research is not conducted in isolation; its social context is critical. Racism is virulent in this country, and it will, like sexism, probably intensify as competition for scarce jobs becomes even more keen. Just as every discovery in molecular biology is examined for its potential for curing cancer, so any biologically based social theory is examined for its possible political and social implications. For these reasons we see an intimate connection between the ideas of group conflict in sociobiology and racist ideology that all of us, including sociobiologists, abhor.

The most striking example of sociobiology's destructive potential is without doubt the film Sociobiology: Doing What Comes Naturally (1976). This film, produced by a private company for distribution in high schools and colleges, concentrates on the biological nature of behavioral differences between the sexes and on aggressive behavior. One horrifying segment shows American planes bombing Vietnamese villages while the sound track suggests that war is the process by which the better genes win out in the struggle for survival, and that the pillage and rape that accompany conquest have biological roots as the conquerors try to spread their genes. This film is so objectionable that the sociobiologists who are interviewed in the film disavow any connection with it. Granting their displeasure with the final version, it should be noted that their statements in the film do not seem to have been taken out of context and that they are not at all at variance with the visual scenes shown while they are speaking. But even supposing that they were unwitting instruments of the producer, can sociobiologists disclaim all responsibility for a film which simply extends standard sociobiological reasoning?

The protestations by sociobiologists that they intend no political or ethical implications in their theory are undercut not only by the film but also by the fact that sociobiologists have publicized their theory by means of articles and interviews in such popular publications as The New York Times, House and Garden, Newsweek, Time, and Psychology Today. It hardly needs to be mentioned that these publications have concentrated on human rather than animal sociobiology. The consequences of sociobiology take two forms. First, the theory, like Jensen's, has implications for official decision making. For example, sociobiology might be considered relevant in deciding whether affirmative action programs for women should be enforced if women are biologically less suited for professional careers. Second, the theory affects the attitudes and prejudices of people as they lead their daily lives. Will they treat others as equals or as inferiors? We are well aware of the distinction between the theory and its vulgarizers, but where does responsibility begin or end? Using any sort of balancing test, the benefits for human knowledge and to society gained by publicizing the highly speculative theory of sociobiology as scientific fact and by encouraging its use by social scientists is in our opinion clearly outweighed by the actual and potential harm resulting from the theory. It seems essential that sociobiologists and those who would use sociobiological concepts be fully aware of the implications of their theorizing. In addition, all of us must take on the responsibility of becoming familiar with the theory, its flaws, its implications, and the results of its use. The social impact of such doctrines as sexism and racism is too great to be ignored.

Acknowledgment: I would like to thank the members of SSG especially J. Beckwith, S. Gould, R. Lange, L. Leibowitz, and F. Salzman for their constructive criticisms of this paper.

BIBLIOGRAPHY

Alper, J., Beckwith, J., and Miller, L. "Sociobiology is a Political Issue." In A.L.Caplan (Ed.), The Sociobiology Debate. New York: Harper and Row, 1978.

Barash, D. P. Sociobiology and Behavior. New York: Elsevier, 1977.

Beckwith, J. and Miller, L. "Behind the Mask of Objective Science." The Sciences, 1976, 16, #6, pp. 16-19, 29-31.

Birket-Smith, K. The Eskimos. (2nd Ed.) London: Methuen, 1959.

Block, N. J. and Dworkin, G. The IQ Controversy. New York: Pantheon, 1976.

Braverman, H. Labor and Monopoly Capital. New York: Monthly Review Press, 1974.

Brown v. Board of Education. 347 U.S. 483 (1954).

Cavalli-Sforza, L. L. "The Genetics of Human Populations," Scientific American, 1974, Sept., 81-89.

Chasin, B. "Sociobiology: A Sexist Synthesis," Science for the People, 1977, 9, #3, 27-31.

Education Development Center. Exploring Human Nature: Cambridge, MA: EDC, 1973.

Feyerabend, P. Against Method. London: New Left Books, 1975.

Goldsby, R. A. Race and Races, (2nd Ed.) New York: Macmillan, 1977.

Gouldner, A. W. The Dialectic of Ideology and Technology. New York: Seabury Press, 1976.

Habermas, J. Toward a Rational Society. Boston: Beacon Press, 1970.

Harris, M. Culture, Man, and Nature. New York: Cromwell, 1968.

Jensen, A. R. "How Much Can We Boost IQ and Scholastic Achievement?" Harvard Educational Review, 1969, 39, 1-123.

Kamin, L. J. The Science and Politics of IQ. Potomac, Maryland: Lawrence Erlbaum Associates, 1974.

Kuhn, T. S. The Structure of Scientific Revolutions. (2nd Ed.) Chicago: University of Chicago Press, 1970.

Lewontin, R. C. "The Apportionment of Human Diversity," Evolutionary Biology, 1972, 6, 381-398.

Maynard-Smith, J. "Group Selection," Quarterly Review of Biology, 1976, 51, 277-283.

Noble, D. F. America by Design. New York: Alfred A. Knopf, 1977.

Sahlins, M. The Use and Abuse of Biology. Ann Arbor: University of Michigan Press, 1976.

Salzman, F. "Are Sex Roles Biologically Determined?" Science for the People, 1977, 9, #4, 27-32.

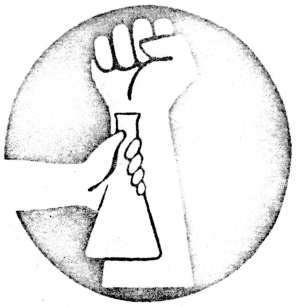
Sociobiology: Doing What Comes Naturally. A film distributed by Document Associates, Inc., 880 Third Ave., New York, New York 10022, 1976.

Sociobiology Study Group of Science for the People. "Sociobiology: A New Biological Determinism," In Ann Arbor Science for the People (Ed.), Biology as a Social Weapon. Minneapolis: Burgess, 1977.

Time Magazine. August 1, 1977.

Wilson, E. O. Sociobiology: The New Synthesis. Cambridge, MA: Harvard University Press, 1975a.

Wilson, E. O. "Human Decency is Animal," The New York Times Magazine, 12 October 1975.



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SOCIOBIOLOGICAL DETERMINISM: THEME WITH VARIATIONS

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of

Science for the People

Published in

Michigan Discussions in Anthropology

Volume 3 Winter 1978

SOCIOBIOLOGICAL DETERMINISM:

THEME WITH VARIATIONS

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The sociobiology debate has been with us for a number of years, and we can perceive various modifications of the original sociobiological position. The "genetic determinism" of E.O. Wilson's formulation of sociobiology (Wilson, 1975a, 1975c) has been criticized by biologists, anthropologists, and others (Allen, *et al.*, 1975; Sociobiology Study Group, 1976, 1977; Sahlins, 1976; Lewontin, 1977, etc.). In response to this criticism, alternative determinisms have been described by Richard Alexander (this volume) and Stephen Emlen (1976, 1978). It is therefore necessary for critics of sociobiology to consider not only Wilson's statements but also the more recent alternatives.

In this article, we will first deal with the genetic determinist aspects of sociobiology. Although we realize that much of this is familiar material to many of our readers, we nonetheless believe that it is helpful to discuss it in some detail. This position, though much criticized, is still asserted by many prominent sociobiologists, including Wilson (1978). Furthermore, an analysis of it illuminates the problems which the alternative determinisms are trying to avoid.

We then proceed to Alexander's "Evolutionary Determinism" and Emlen's "Ecological Determinism," based on an idea which Gould has recently categorized as "the fallback position" (1978). This approach may well become more popular as the debate continues, but we will argue that it shares many of genetic determinism's basic weaknesses--vagueness, methodological errors, and lack of a strong theoretical base.

Next, we will briefly compare modern sociobiological theories with Social Darwinism. This comparison serves to bring out the historical relativity of theories of society based on the biology of individual behavior, whether called genetic, evolutionary, ecological, or some other type of biological, determinism. Furthermore, we will examine the consequences of such attempts to "biologicize"

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social phenomena, in order to reveal the underlying political and ideological framework of science.

Sociobiology and Genetic Determinism

The genetic determinism of sociobiology has had a variety of manifestations. The best way to describe the kinds of genetic determinism involved is to let the sociobiologists speak for themselves. E.O. Wilson, for example, has stated that

> there are certain limitations and strong biases in the development of human behavior. We are not preordained to one specific behavior, but we are preordained to a definable range of behavior. [1975b:15]

W.D. Hamilton's view of the causes of racism certainly paints him as a genetic determinist:

> I hope to produce evidence that some things which are often treated as purely cultural in man--say racial discrimination--have deep roots in our animal past and thus are quite likely to rest on direct genetic foundations. To be more specific, it is suggested that the ease and accuracy with which an idea like xenophobia strikes the next replica of itself on the template of human memory may depend on the preparation made for it there by selection--selection acting, ultimately, at the level of replicating molecules. [1975:134]

Trivers and DeVore, in the film "Sociobiology: Doing What Comes Naturally," have made such statements as:

> One of the most striking characteristics of warfare, and certainly classical warfare, is that when you overrun the other country, you loot and pillage, but you also grab up the women, and you either inseminate them on the spot or you take them back as concubines. You kill off the adult males; you sometimes castrate young boys and bring them back as servants. So I think warfare has traditionally had a strong sexual counterpart to it, which is certainly biological, and you don't have to look far to see that there's that tendency running today. [Trivers]

> You don't have to be a scientist to notice that among humans, men are much more interested in status and in politics than women are; and it's an interesting question to ask whether there's

> anything in our background that might have led to this, or whether it's just an outcome of human social institutions. And to me, the answer is quite clear: wherever one looks throughout the vertebrates, all the animals, the primates and so on, one finds men competing for status with each other. [DeVore]

Barash tells us that:

> Like the male hoary marmots inhabiting a highly social environment (Chapter 7), the human male can maximize his fitness by interacting with other adults. By competing with other males, he can retain access to his female and also possibly attract additional mates. This line of reasoning thus provides further support for the "biology of the double standard" argument presented above, and it also suggests why women have almost universally found themselves relegated to the nursery while men derive their greatest satisfaction from their jobs. [1977:301]

And Dawkins almost caricatures the position by stating:

> Now they (genes) swarm in huge colonies, safe inside gigantic lumbering robots, sealed off from the outside world, communicating with it by tortuous indirect routes, manipulating it by remote control. They are in you and me; they created us, body and mind; and their preservation is the ultimate rationale for our existence. They have come a long way, those replicators. Now they go by the name of genes, and we are their survival machines. [1976:21]

These statements demonstrate a range of genetic determinisms, from those regarding us as "preordained" or "programmed" by our genes, to those speaking of genetic "biases," "tendencies," or "limits," to those merely implying the causal role of a "genetic basis." However, they all treat the genotype as determining the basic range of options for human behavior, with environmental influences selecting among these options to a greater or lesser extent. To use a metaphor presented by E.O. Wilson, the phenotype is like a ball rolling down a genetic landscape, with environment pushing it out of some channels into others. Some channels in the genetic landscape are deep, so that few kinds of environments can push us out of them, while others are quite shallow (Wilson, 1978). This view and the various degrees of determinism flowing from it, share a basic misconception: that "genotype" and "environment" are separable in terms of their causal action, and thus that genotype

can be the prior and more fundamental determinant of phenotype. Consequently, we must begin our discussion with a critique of the notion that human behavior is "genetically determined."

It is clear from modern genetics that genes as well as environment are involved in every aspect of the phenotype, including behavior. It is equally clear that they normally do not "predispose" or "bias" development toward preferred phenotypes; rather the resulting phenotype depends on what environment the genes grow up in. Without knowledge of both genotype and environment, we cannot predict the phenotype. Thus to say that human genes are "predisposed" or "preordained" to a determinable range of behaviors is like saying that a rectangle whose width is five centimeters is "predisposed" to have an area of about ten square centimeters. Unless we know that the length has to be about two centimeters, this is clearly nonsense. It is equally unscientific to assert that, as Wilson states, "I see maybe ten percent of human behavior as genetic and ninety percent environmental" (Rensberger, 1975:E-36). He could just as well have said two percent or eighty percent or fifty percent is genetic; it is like trying to say what percentage of the rectangle's area is due to its width. In fact, it makes even less sense, for at least we know what "ten percent of the rectangle's area" means, but the phrase "ten percent of human behavior" is scientifically meaningless.

Geneticists study the interaction of genes and environment, not by using such unscientific terms as "genetically predisposed" or "ten percent of human behavior," but by experimentally manipulating genotype and environment and seeing what phenotype results. This method, much used with such organisms as fruit flies, gives us what is called the "norm of reaction"--the pattern of expression of a genotype in several environments (Lewontin, 1976). However, its application to humans has a major drawback--it requires the experimenter to manipulate both the mating patterns and the environments of the subjects. Such a procedure is totally unethical with humans, and no technical advances in research methodology can change this. This is the reason that sociobiologists cannot cite genetic evidence for their hypothetical "genetic biases," nor are they likely to.

What kind of evidence do sociobiologists offer us to prove that a behavior is "genetic"? One method is to look for behaviors found universally in all cultures. Universality, though, does not show that a behavior is genetic. People in essentially every human culture wear clothes. Does that mean that we have a gene for wearing clothes? Even if the universality of a trait were sufficient evidence for genetic determination, the sociobiologists' arguments would remain unsupported because they rest on distortions of the available data. They have drawn selectively on certain pieces of ethnographic data to support their assertions, while completely ignoring those which refute them. For example, the universality of warfare is a prime tenet of sociobiology. This is in direct contradiction to ethnographic evidence. The absence of any form of warfare has been reported among the Andaman Islanders, the Arunta,

the Eskimos, the Mission Indians, the Semai, the Todas, the Western Shoshone and the Yahgan. The Tasaday of the Philippines do not even have a word for war. In societies where it does occur, warfare often consists of threats, insults, and single combat between aggrieved individuals. Rarely are more than a few individuals killed in an episode of warfare; in some cases, such as that reported among the Tiwi of northern Australia, the war is stopped as soon as one person is wounded (Fried, *et al.*, 1968; Durham, 1976b; Nance, 1975).

Another key element in the sociobiologists' view of human nature is the universality of sex roles as we know them. Again, this is clearly refuted by ethnographic data. Indeed, decades have passed since Margaret Mead's classic study, *Sex and Temperament in Three Primitive Societies*, described one society in which both sexes conform to our ideal of the "feminine" role, another where both sexes fit our "masculine" norm, and a third in which women are "masculine" and men are "feminine." Nor does sexual division of labor invariably show the "man, the hunter; woman, the homemaker" pattern set out by sociobiologists. Furthermore, there is little or no division of labor by sex among either the Pygmies of the Ituri forest or the Tasaday. Nor are males always more aggressive, another component of the sociobiological scenario. Among the Pygmies and Tasaday, both sexes are nonaggressive. Recent visitors to China also report that children of both sexes are nonaggressive (Reiter, 1975; Fernandez and Lynch, 1972; Nance, 1975; Turnbull, 1968).

Sociobiologists' use of primate behavior as evidence of genetic determination of human traits is equally misleading. Patterns of aggression, dominance, sex roles, and other features of social organization vary greatly even among primates of the same species. Baboons living on the savanna, which are the most frequently cited models of primate behavior, display a rigid hierarchy, male dominance, and extreme aggressiveness. There is, however, another side to the story which sociobiologists have managed to obscure. Forest baboons show little aggression and no male dominance hierarchies. In times of danger, the males run up trees, leaving the females to protect the young. Adult females frequently direct troop movements as well. Clearly, males are neither more dominant nor more aggressive in these groups. Similar behaviors have been found among chimpanzees and mountain gorillas, who are our closest relatives evolutionarily. These groups also display a lack of both aggressive behavior and rigid male dominance hierarchies (Reiter, 1975; Kummer, 1971).

According to Wilson (1977), behaviors which are found in other primates are part of our basic genetic primate or mammalian heritage, and thus must be genetic. Ironically, however, if behaviors *can't* be found in other primates (*i.e.*, if they are "species-specific" to humans), it is argued that they are "so distinctly and ineluctably human, that they can be safely classified as genetically based" (Wilson, 1975c:48). Thus, either presence *or* absence of supposed analogues of human traits among primates is regarded as evidence of their genetic basis. Beyond this contradiction, it must be realized

that the analogy from primate behavior to our own is too often a consequence of our biased perception of their behavior, which results from projecting our social interactions onto nature and then turning around and describing our behavior as "natural" (Bookchin, 1977).

The adaptiveness of a behavior is often given as the final piece of "proof" of its genetic basis. If you can figure out some way a behavior could possibly benefit an individual or his/her genes, sociobiologists will claim that it is due to the action of natural selection operating over evolutionary time. This is the sociobiologists' favorite pastime: to think up adaptive explanations for even the most bizarre behavior. For example, why is it adaptive for children to dislike spinach? DeVore, *et al.*, in a high school text (1973), remind us that spinach has oxalic acid in it, which combines with calcium and may inhibit bone formation. Children may therefore have a genetically-based dislike for the vegetable. Perhaps they also like to eat candy because it is really, genetically, the best thing for them to eat. But even when you see behaviors that could be defined as maladaptive, these too are genetic; they are just being selected against. In discussing the problem of divorce, DeVore, *et al.*, explain it as a "holdover" from prehuman history: "Perhaps natural selection has simply not had enough time to perfect the human pair bond." And, if all else fails, you can claim that the behavior was adaptive during the Pleistocene, even though it isn't now.

This variety of methods used to show adaptiveness is vulnerable to serious criticism. But beyond this, a more fundamental problem is that human societies can arrive at adaptive solutions by *cultural* means, as well as by natural selection (Durham, 1976a).

Thus sociobiologists' criteria for deciding whether a behavior is genetic are so loose that one can claim that essentially any behavior has a genetic basis. And indeed they have made such claims for a list of behaviors that is increasing every day, to include such recent additions as "economic behavior" (Wilson, 1977) and "space travel" (Yinger, 1977). But what do sociobiologists mean when they say that something has a genetic basis? The common notions, that it is natural, preordained, inevitable or difficult to change, are either false or not well-defined. Certainly everything we do has a genetic basis in the sense that if we didn't have any genes, we wouldn't have any behavior. Further, an individual with the genes of a frog or a tapeworm would presumably exhibit much different behavior than the rest of us, even if it were brought up in the same social environment (whatever that means). So what do we learn by knowing that a behavior is genetic? Only that frogs and tapeworms are not likely to exhibit human-like social behavior? No, sociobiologists claim that from genetic knowledge we learn of our "true nature" and can learn to better overcome its tendencies. But if this is the promise of sociobiology, it is a hollow one. Does knowledge of a genetic basis really help us to change a behavior? PKU disease, which is due to a single mutation, produces severe brain damage if the person's diet includes phenylalanine,

but no bad effects at all if a diet lacking phenylalanine is provided. The knowledge of the cure was not a result of the knowledge of the genetic basis. On the other hand, we have no idea how to cure Tay-Sachs disease, also known to be caused by a single mutation (Lewontin, 1976). There is no reason to believe that knowledge of a "genetic basis" for racism or warfare will clue us in to the environmental or social changes that we should make to alleviate these problems.

A New Determinism?

The above discussion is principally concerned with analyzing arguments of that group of sociobiologists (*e.g.*, E.O. Wilson) who claim either that: (1) there are complex human social behaviors common to all people, and these behaviors collectively constitute a genetically based "human nature" which may be difficult to change; and/or (2) variations in gene frequencies are important in determining differences in significant aspects of human social behavior between groups and between individuals. Several sociobiologists, though, disagree with Wilson on the above issues, and in fact R.D. Alexander hypothesizes that

> the vast bulk of cultural variations among peoples alive today will eventually be shown to have virtually nothing to do with their genetic differences. [1977a:6]

He also states that we can best eliminate pernicious human behaviors "by changing the *environment* once we have discovered, by analysis, what the causative *environment* is" (this volume; emphasis added). Alexander believes that the better we understand ourselves, the better we can change ourselves, and "it is environment, not genes, that we are best at changing for the better" (this volume). He describes an "Evolutionary Determinism," which he contrasts to genetic determinism.

Although Alexander's position appears at first to be significantly different from genetic determinism, he has at the same time repeatedly insisted that a "Darwinian model seems to me to be established beyond a doubt as an appropriate hypothesis for the background of a significant, if not the major, fraction of the proximate behavioral tendencies and motivations of humans" (1977b:332-333). He further claims that the Darwinian model will be very useful in "explaining the backgrounds and manipulability of our inclinations and motivations" (this volume). We think that Alexander's position embodies a fundamental inconsistency, and if not, it leaves us with no clear guidelines for how to proceed either as critics or as investigators. For instance, to the extent that Alexander does explain his methodology, it tells nothing about how we might discover the degree or kinds of manipulability that exist.

What methodology does Alexander use to study human social behavior? His basic method is to make predictions about human social

behavior based on organic evolution, and to then look in the ethnographic literature to see if the behavior of different cultures supports his hypothesis. For example, evolutionary theory predicts that when there is very low confidence of paternity, a male will give more parental care to his sister's offspring than to the children born of his wife (Alexander, 1977b). If cultural data correspond to the evolutionary predictions, it is concluded that these behaviors are the result of the selective pressures of the environment on the behaviors that existed in the past, acting so as to increase inclusive fitness. "I cannot imagine any single hypothesis, other than a history of natural selection of genetic alternatives, that could account for a general patterning of human behavior which fits the Darwinian model in many different societies and circumstances" (Alexander, 1977b:305).

Although Alexander insists that this *genetic* model can explain the background and manipulability of human behaviors, at the same time he says that the differences in behavior between groups of people are not likely to be related to genetic differences, and that we can best change human behaviors by changing the environment. Alexander's use of a Darwinian model without a direct emphasis on genetic determinism leads us to a view similar to Emlen's "Ecological Determinism" (Emlen, 1976, 1978).

Emlen attempts to escape the problems of genetic determinism by arguing that whether the characteristics of social systems are transmitted genetically or culturally, they will take the forms predicted by sociobiological theory. These social structures are the adaptive solutions to the ecological problems presented by the environment, and thus a given type of environment will lead to a given type of social system. Emlen has apparently independently reinvented ideas which were long ago discussed and transcended in anthropology and geography--the "environmental determinism" of Huntington and the later "possibilist" or "limitationist" modifications of this in the early 1950's. Recent work in anthropology has indeed made major advances in relating ecological factors to culture (*e.g.*, Rappaport. 1968)--but it has been able to do so precisely because it has gone beyond the idea that environment determines or limits the development of culture. In fact, we now recognize that "ecological factors" such as resource distribution, a major variable in Emlen's formulation, cannot be considered independently of culture. Rather it depends greatly on what is defined as a resource, what kind of technology is in use, and what kinds of production relations exist, variables which may vary independently of the environment (Harvey, 1974).

The fundamental problem with theories such as Emlen's and Alexander's is that despite their reliance on the Darwinian model, they are in fact non-Darwinian. These theories assume that fitness will be maximized, as in the Darwinian theory of evolution by natural selection. But fitness is, by definition, measured by gene frequencies in future generations; if the behavior arises and is transmitted nongenetically, there is no theoretical link between behavior and fitness. The assumption that fitness will be maximized

is completely *ad hoc*. The predictive strength of the theory of natural selection lies in the fact that if different behaviors are correlated with different genes *and* lead to differential survival, then changes in gene frequency will occur in the population, leading to certain behaviors (those with highest genetic fitness) becoming predominant. This theoretical basis is totally lost if genes are removed from the model. Thus the claim that we will nonetheless see the same results as if we were dealing with natural selection acting on genes, becomes totally arbitrary. The results *may* be the same, of course, but we no longer have a powerful theory that predicts that they *must* be. Genetic determinism is avoided, but only at the expense of losing the power of Darwinism. Thus sociobiologists have a problem that they will not be able to solve merely by invoking a different kind of determinism.

If genes, then, are not important in this model, what ultimately *is* transmitted? Acquired characteristics. This is not a new idea; Lamarck introduced it nearly two centuries ago. It is unfortunate for those who wish to use the Lamarckian model--as anyone who wishes to pursue an evolutionary analysis of characteristics that are not based on genes does--that a solid theoretical foundation for such an assumption does not appear to be available. This state of affairs is probably the result of the almost universal agreement that organic evolution is able to happen because of the genetic foundation upon which the evolving characteristics rest--an agreement reached about 1900. In the case of cultural evolution, Lamarckianism becomes yet more complicated. Not only are acquired characteristics passed on, but to whom they are passed need not be a function of genealogical relationship.

A rigorous theory of Lamarckianism has not been developed, and a theory of cultural evolution, although hotly debated in anthropology, still eludes us (see Durham, 1976a, and Feldman and Cavalli-Sforza, 1976, for further discussion). But the real point is that speculations based on classical Darwinism are irrelevant. They will not necessarily be incorrect, but they simply do not apply to the case being considered. It might be likened to attempting to apply Newtonian mechanics to the process of uranium enrichment, or epicycles to spinning electrons.

The various determinisms proposed by sociobiologists, while differing in their degree of emphasis on genes, share the property that they describe social systems as the sum of the fitness-maximizing behaviors of individuals. Even group-selection models have tended to focus on the proportions of *individuals* in a group displaying altruistic or non-altruistic behaviors. Therefore, beyond criticizing the details of each model, it is necessary to consider the basic approach to social science that is implied by all of them. An historical perspective is useful here in considering the prospects of social models based on the biology of individual behavior.

Sociobiology in Perspective

Sociobiology has been described as a new set of revolutionary theories. We have criticized these theories on grounds of clarity of formulation, consistency, methodology, scientific validity, and applicability (see also Gould, 1978; Lewontin, 1976, 1977; Sociobiology Study Group, 1977). Beyond that, we claim that they are not particularly new. Sociobiology is a recent variant of the Social Darwinism which was so prominent around the turn of the century. It might be argued that sociobiology is a science, not a vulgarization of biology like Social Darwinism. But Social Darwinism was considered a science in its time. Its output filled massive books with impressive scientific evidence on racial, sexual, and ethnic differences, and it numbered among its practitioners many of the most respected names in contemporary science. Social Darwinism, and the eugenics movement founded upon it, also claimed to have found revolutionary new tools of analysis, such as anthropometric parameters and the IQ test, with its finding that 79% of Italian immigrants, 89% of Poles, and 83% of Jews were "feebleminded" (Kamin, 1974; Hofstadter, 1955; Shields, 1978; Woodward, 1977).

Social Darwinism reflected the ideology of its time and its form changed with the times. Toward the end of the nineteenth century, the competitive individualism of the earlier Social Darwinist formulation gave way to a more holistic view of biologically determined social structure. As the corporation became the model, not only for business, but also for society, the corporate emphasis on efficient cooperation replaced the entrepreneurial, competitive, individualistic image of both economic and social life. Not surprisingly, the same transformation occurred in the view of the social and biological organization of animals (*e.g.*, W.B. Cannon's homeostatic view of both organisms and society (1939)). The political and economic reality of the time was labor/capitalist confrontation and the threat of socialism. The corporate ideal, with its stress upon efficient production, formed part of a social reform movement based on undercutting discontent and blending people into a harmoniously functioning unit. The other "revolutionary" sciences of scientific management and industrial psychology were part of the same movement (Braverman, 1974). Sociobiology could easily be analyzed in similar terms.

The historical analysis of these variants of "social biology" shows us the way science "naturalizes" ideologies, so that their social origins and functions are obscured. We maintain that such biology is inherently political despite claims to scientific objectivity. To emphasize tendencies, motivations, or whatever formulation of a softened individual determinism is used, is to strip a social/political/economic process of its essential nature. Social phenomena cannot be studied at the level of the individual. One might study the individual with dispassionate objectivity, but the use of explanations gained to characterize the higher level is more than an epistemological mistake. Only political conviction, conscious or unconscious, can sustain it. In 1934, Wilhelm Reich

criticized psychoanalysis in the same way. If we substitute sociobiological terms for psychoanalytic ones, we could easily adapt his critique to sociobiology:

> But if one insists upon using the results obtained by psychoanalyzing individual workers to explain the phenomenon of the strike, then the conclusion is inescapable that a strike is a revolt against the father. The fact that in reaching this conclusion one identifies "strike" with "psychical behavior in a strike" passes unnoticed; yet the difference is decisive. It passes unnoticed either because of methodological confusion or for conscious or unconscious reactionary motives, for the conclusions to be drawn from the sociological interpretation are not the same as those to be drawn from the psychological interpretation. The former leads to recognizing the laws of class society, the latter to obscuring those laws. [1972:71]

The denial of the specifically social/political/economic character of a process and of the level of the explanation appropriate to it is a politically conservative act. The issue is not one of scientific objectivity versus political bias, but of the reformulation of the terms in which one is expected to think of human potential for change. The consequence of this reformulation is to obfuscate the political nature of social change. We have criticized sociobiologists for implying that there are serious limits to the ways that we can change society and that there is some danger in pursuing these changes without the guidance of evolutionary theory. We further object to the implicit identification of political phenomena with individual behavior supposedly determined by fitness. This identification exemplifies the ideological content of scientific ideas. Ideologies are not always the conscious constructions of individuals. If they were, they would always be out in the open political arena, where we could respect and criticize them as sincerely held beliefs. In dealing with sociobiology, our task is not merely to eliminate bad science, but also to recognize the implicit political content of science. Only in this way can science become an explicit part of the political struggle for social change.

BIBLIOGRAPHY

Alexander, R.D.
1977a Evolution, Human Behavior, and Determinism. *Proceedings of the Biennial Meeting of the Philosophy of Science Association* (1976) 2:3-21.

1977b Natural Selection and the Analysis of Human Sociality. In *The Changing Scenes in the Natural Sciences, 1776-1976*, ed. C. Goulden, pp. 283-337. Philadelphia Academy of Natural Sciences Special Publication 4.

Allen, Elizabeth, *et al.*
1975 Against "Sociobiology." *New York Review of Books* 22: 43-44.

Barash, D.
1977 *Sociobiology and Behavior*. New York: American Elsevier.

Bookchin, M.
1977 Ecology, Society, and the Myth of Biological Determinism. In *Biology as a Social Weapon*, eds. Ann Arbor Science for the People, pp. 123-129. Minneapolis: Burgess.

Braverman, H.
1974 *Labor and Monopoly Capital*. New York: Monthly Review Press.

Cannon, W.B.
1939 *The Wisdom of the Body*, revised edition. New York: W.W. Norton.

Dawkins, R.
1976 *The Selfish Gene*. New York: Oxford University Press.

DeVore, I., R. Trivers and G. Goethals
1973 *Exploring Human Nature*. Cambridge, Mass.: Educational Development Corporation.

Durham, W.H.
1976a The Adaptive Significance of Cultural Behavior. *Human Ecology* 4:89-121.

1976b Resource Competition and Human Aggression, Part I: A Review of Primitive War. *Quarterly Review of Biology* 51:385-415.

Emlen, S.T.
1976 An Alternative Case for Sociobiology. *Science* 192:736-738.

Emlen, S.T.
1978 Ecological Determinism and Sociobiology. Paper presented at the A.A.A.S. meetings, Washington, D.C.

Feldman, M.W. and L.L. Cavalli-Sforza
1976 Cultural and Biological Evolutionary Processes, Selection for a Trait under Complex Transmission. *Theoretical Population Biology* 9:238-259.

Fernandez, C.A. and F. Lynch
1972 The Tasaday: Cave-dwelling food gatherers of South Cotobato, Mindanao. *Philippine Sociological Review* 20: 275-330.

Fried, M., M. Harris and R. Murphy, eds.
1968 *War: The Anthropology of Armed Conflict and Aggression.* Garden City, New York: Natural History Press.

Gould, S.J.
1978 Sociobiology and the Theory of Natural Selection. Paper presented at the A.A.A.S. meetings, Washington, D.C.

Hamilton, W.D.
1975 Innate Social Aptitudes of Man: An Approach from Evolutionary Genetics. In *Biosocial Anthropology*, ed. R. Fox, pp. 133-155. New York: Halsted Press.

Harvey, D.
1974 Population, Resources, and the Ideology of Science. *Economic Geography* 50:256-277.

Hofstadter, R.
1955 *Social Darwinism in American Thought.* Boston: Beacon Press.

Kamin, L.
1974 *The Science and Politics of IQ.* New York: Halsted Press.

Kummer, H.
1971 *Primate Societies: Group Techniques of Ecological Adaptation.* Chicago: Aldine.

Lewontin, R.C.
1976 The Fallacy of Biological Determinism. *The Sciences* 16:2.

1977 Caricature of Darwinism. *Nature* 266:283-284.

Nance, J.
1975 *The Gentle Tasaday.* New York: Harcourt Brace Jovanovich.

Rappaport, R.
1968 *Pigs for the Ancestors.* New Haven: Yale University Press.

Reich, W.
1972 *Sex-Pol Essays 1929-1934.* New York: Lee Baxandall.

Reiter, R.
1975 *Toward an Anthropology of Women.* New York: Monthly Review Press.

Rensberger, B.
1975 The Politics in a Debate over Sociobiology. *New York Times,* November 9, 1975, p. E-36.

Sahlins, M.
1976 *The Use and Abuse of Biology: An Anthropologist's Critique of Sociobiology.* Ann Arbor: University of Michigan Press.

Shields, S.A.
1978 Nineteenth Century Evolutionary Theory and Male Scientific Bias. Paper presented at the A.A.A.S. meetings, Washington, D.C.

Sociobiology Study Group of Science for the People
1976 Sociobiology--Another Biological Determinism. *Bioscience* 26:182-186.

1977 Sociobiology--a New Biological Determinism. In *Biology as a Social Weapon,* eds. Ann Arbor Science for the People. Minneapolis: Burgess.

Turnbull, C.
1968 The Importance of Flux in Two Hunting Societies. In *Man the Hunter,* eds. R.B. Lee and I. DeVore, pp. 132-137. Chicago: Aldine.

Wilson, E.O.
1975a *Sociobiology: the New Synthesis.* Cambridge, Mass.: Harvard University Press.

1975b Quoted in *National Observer,* August 16, 1975, p. 15.

1975c Human Decency is Animal. *New York Times Magazine,* October 12, 1975, pp. 38-50.

1977 Biology and the Social Sciences. *Daedalus* 106(4):127-140.

1978 Trends in Sociobiological Research. Paper presented at the A.A.A.S. meetings, Washington, D.C.

Woodward, V.
1977 IQ and Scientific Racism. In *Biology as a Social Weapon*, eds. Ann Arbor Science for the People. Minneapolis: Burgess.

Yinger, R.
1977 Exosociology and Human Evolution. Paper presented at the American Sociological Association meetings, Chicago, Illinois.

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Sociobiology: A Sexist Synthesis

Barbara Chasin — *Science for the People* — *May-June 1977*

Are Sex Roles Biologically Determined?

Freda Salzman — *Science for the People* — *July-Aug. 1977*

Sociobiology: A Sexist Synthesis

Barbara Chasin

During the 1960's — a time of great social unrest, questioning of basic American institutions, and a growing interest in socialism as an alternative — there appeared a spate of books on the theme that humans are only another species of ape. Konrad Lorenz, Robert Ardrey, Desmond Morris, Lionel Tiger and Robin Fox, as well as lesser known writers, try to convince their readers that animal studies are the key to an understanding of human social behavior.(1) War, violence, private property, inequality: all of the major negative features of capitalism are said to be natural and inevitable legacies from our primate origins.

We are descended from an aggressive species. Man emerged as a hunting animal ("man" is the correct word here). One of the main themes of the biological determinists is that the males of both the human and primate species carry the trait of aggressiveness. Male primates do the fighting, the protecting, the hunting, and; among humans males engage in analogous activities. From our earliest days, so the story goes, the man was the active, aggressive, subsistence-providing person, while the little woman cleaned the cave, cooked the mastodon and reared the kiddies. A charming picture but, in all probability, completely false. These authors *create* a never-never land, which they then "explain" with allegedly hard-headed science.

Biological determinism has been given a somewhat more sophisticated tone by Edward O. Wilson, Harvard biologist.(2) Wilson adds a veneer of genetics and mathematics to the crude assertions of his predecessors. Yet beneath the surface lies the same tired refrain, that our destiny is controlled by our biology.

In his magnum opus, *Sociobiology: The New Synthesis*, Wilson informs us that both the nuclear family and a sexually based division of labor are universal phenomena:

> The building block of nearly all human societies is the nuclear family. The populace of an American industrial city, no less than a band of hunter-gatherers in the Australian desert is organized around this unit..... During the day the women and children remain in the residential area while the men forage for game or its symbolic equivalent in the form of barter and money. The males cooperate in bands to hunt or deal with neighboring groups. If not actually blood relations, they tend at least to act as 'bands of brothers'.(3)

The same point is formulated in slightly different fashion, in Wilson's article in the *New York Times Magazine*:

> In hunter-gatherer societies, men hunt and women stay at home. This strong bias persists in most agricultural and industrial societies and on that ground alone, appears to have a genetic origin. No solid evidence exists as to when the division of labor appeared in man's ancestors or how resistant to change it might be during the continuing revolution for women's rights. My own guess is that the genetic bias is intense enough to cause a substantial division of labor even in the most free and most egalitarian of future societies.(4)

Sociobiologists and Aggression

In his *New York Times* article Wilson cites a book by Eleanor Maccoby and Carol Jacklin, *The Psychology of Sex Differences*. The intent of the authors is "to sift the evidence to determine which of the many beliefs about sex differences have a solid basis in fact and which do not."(5) They conclude that there are two areas in which biological differences between men and women result in different behavioral characteristics. One is spatial-visual skills (which won't be discussed here). The other is aggression. Simply put, Maccoby and Jacklin claim that males are more aggressive than females and that this is biologically based.

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Most biological determinists use the notion of the greater aggressiveness of males fairly consistently to explain the "fact" that men are the doers, the makers of history. For example, Wilson concludes that even when men and women have "identical education and equal access to all professions, men are likely to play a disproportionate role in political life, business and science"(6) because of this difference in aggressiveness.

In their characterization of the relations between sex and aggression, Maccoby and Jacklin cover the range of arguments used by biological determinists on this issue. For this reason, and because Wilson uses Maccoby and Jacklin as evidence for his own assertions, a critique of their discussion is important.

To begin with, what they mean by aggression is never made very clear. (That is a general problem in examining this phenomenon: what is actually being talked about?) One central theme which Maccoby and Jacklin discern in aggressive behavior is the intent of one individual to hurt another; but, as we shall see, their definition changes when the data do not correspond to this particular formulation.

They cite four kinds of evidence for a biological basis for male aggressiveness: 1) a relation between aggression and levels of sex hormones, 2) similar sex differences can be found in man and subhuman primates regarding aggression, 3) sex differences in aggression are found early in life at a time when differential socialization cannot be occuring, and, 4) males are more aggressive than females in all human societies for which evidence is available.

Let us look briefly at each of these arguments.

1). Their evidence for the importance of sex hormones comes mainly from studies of rats and monkeys. Since humans consciously control their behavior to an extent unimaginable in a rodent or even a monkey, it seems very unsound to assume, where social interaction is concerned, that as animals do so do humans.

Looking at people, Maccoby and Jacklin do, in fact, admit that little is known about the relation between sex hormones and behavior. They refer to two studies. One is a report on testosterone levels of 21 young men in prison. The men with higher levels of this male hormone had allegedly committed more violent and aggressive crimes during adolescence. But to make their point, the authors of this particular study have to resort to a curious definition of aggression. Aggression, for them, includes not only such acts as murder and assault but *escape from institutions*.(7)

The most generous thing you could say about this research is that it is inconclusive. Furthermore, even if a correlation exists, the direction of causation is not therefore obvious. Hormone levels themselves have been shown to change as a result of experiences.(8) Maccoby and Jacklin do not even discuss this possibility.

The other study they use has a certain ludicrous quality. Seventeen fetally androgenized girls were compared to their eleven normal sisters. Fetally androgenized means that they received excess amounts of male hormones when they were fetuses. They had masculinized genitalia which were surgically corrected. But even after surgery "their behavior continued to be masculinized in the following ways: they much more often preferred to play with boys; they took little interest in weddings, dolls, or live babies, and preferred outdoor sports."(9) However, they did not fight significantly more than their sisters did.

We are asked to believe, on the basis of this tiny sample, that an interest in such things as dolls, weddings, live babies and sports is linked to our hormonal make-up. This is a highly dubious proposition. The girls, it should be remembered, were born with male genitalia and their parents were aware of their sexual ambiguity. This could have influenced their interaction with these daughters — but this possibility is not discussed.

2). The evidence on primates contains a great deal of oversimplification. Primates, even of the same species, differ from one another in their patterns of aggression, dominance, sex roles, etc. Baboons living on the plains do show the classic pattern of dominant males making decisions regarding troop movements, having privileged access to food and sex and acting as the protectors of the young. (These animals are featured prominently in a film on sociobiology entitled "Doing What Comes Naturally.") But what sociobiologists and Maccoby and Jacklin don't tell us is that Forest baboons display little aggression and no male dominance hierarchies. When troops meet, which is rare, the encounters are friendly. When danger is perceived, the males run up the trees leaving the females and young to deal with things on their own. The adult females are more likely to direct troop movements than are the males.(10) Thus, it is not at all clear that male primates are more dominant or aggressive than females.

But even if they were, we would not have an explanation of human social behavior. The behavior of primates does not automatically explain ours.

3.) Maccoby and Jacklin's third point — that sex differences regarding aggression are found so early that socialization can't account for them — is very difficult to believe. What is aggressive behavior in a new-born infant? Furthermore, there is evidence that differential behavior towards babies based on their sex seems to occur very soon after birth.(11)

4). Finally, there is the most important question of cross-cultural evidence. If we find that males are more aggressive than females in all human societies, there would be some reason to think this is a sex-linked characteristic. Maccoby and Jacklin use only two studies. One has to do with playground behavior in the United

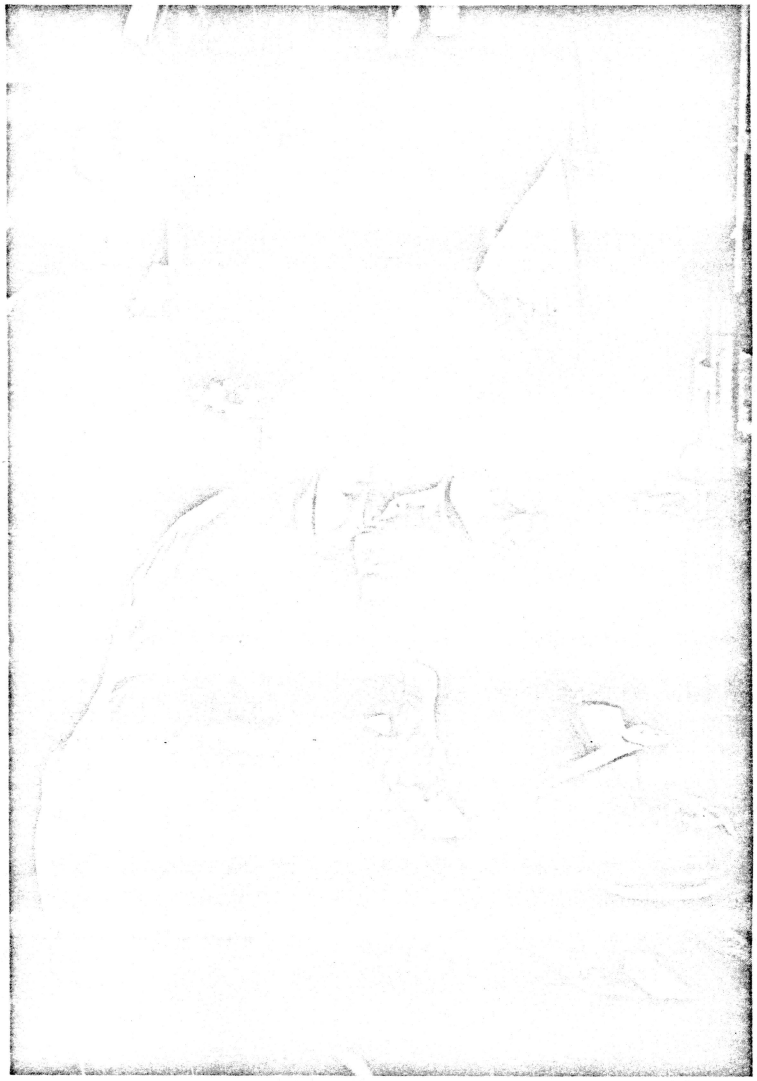

Photo by Marc Riboud

States, Switzerland and Ethiopia. Boys more often hit or push each other without smiling in each of these societies. The other cross-cultural evidence they use is based on Whiting's and Pope's discussion of data from six cultures. Maccoby and Jacklin themselves note that in this material there were few physical assaults of children upon one another and sex differences didn't account for these. But boys were more likely to engage in "rough and tumble play" (which is now synonymous with aggression; remember, the earlier definition was an intent to hurt), to have more verbal insults among themselves and to counterattack if physically or verbally assaulted. These studies of societies where boys and men are more likely to engage in some type of aggressive behavior than are girls or women hardly prove than men are *innately* more aggressive than women.

In each of these four arguments there is something rather dishonest, and this shows up rather consistently in the work of biological determinists. They simply do not discuss data that is inconsistent with their statements.

There are societies where neither sex is aggressive. Preliminary reports on the Tasaday of the Phillipines have noted the gentleness of males and females and their lack of anything resembling fighting. There is no war, no word even for war, nor is there a sexual division of labor. Such leadership as there is has at times been exercised by a woman.(12) In pygmy society too it is hard to find examples of males being more aggressive than females.(13) Ruth Sidel's descriptions of children in socialist China are worth citing here as well:

> The emphasis on the People's Liberation Army and on defending the motherland stands in sharp contrast, however, to the lack of aggression you see in the children in their day-to-day life. That we never saw a child push another child, never saw a child grab a toy from another child, never saw any hostile interaction between children or between adults and children truly amazed us. When we asked about aggression at a kindergarten at the workers' village in Shanghai, we were told by the kindergarten teacher, Lu Shiutsung, that aggression is not a problem, because the children have already "received collective training in nursery." She allowed that occasionally a child might be aggressive, but this usually can be handled through "education."(14)

Continuing, she notes:

> What is so amazing, of course, in walking the streets of Peking or Shanghai, or visiting a commune or urban neighborhood, is that we never saw aggression among the children. No doubt it exists, but we never witnessed it. At one park in Hangchow, one of us handed a piece of candy to a boy of about ten; he immediately passed it on to his baby sister. He was then handed a second piece, which he passed to his mother. He kept the third piece; he had no one else to give it to.

Felix Greene noticed the same lack of aggression in children during his trip: "I have spent a lot of time watching children playing on the streets — little tots all on their own. They are endlessly in-

ventive in their games — a piece of wood or a bit of string will keep them happy for hours. They never fight! *Why* don't they? They never snatch — never 'That's *mine!*' "(15)

Sociobiology and Sex Roles

The cross cultural evidence on sex roles is crucial and largely ignored or misrepresented in the works of the sociobiologists. There are societies, and one can argue that these were the typical human groupings for millenia, where there is little division of labor. But even where some division exists, it is far different from that portrayed by Wilson *et al.* Men and women may engage in different tasks but women are not confined to puttering around the campfire all day doing domestic chores.

Colin Turnbull spent several years living with the Pygmies of the Ituri forest in the Congo. He writes:

> Between men and women there was... a certain degree of specialization, but little that could be called exclusive.(16) The woman is not discriminated against... She has a full and important role to play. There is relatively little specialization according to sex. Even the hunt is a joint effort. A man is not ashamed to pick mushrooms and nuts if he finds them or to wash and clean a baby. A woman is free to take part in the discussions of men if she has something relevant to say.(17)

Wilson does not make any reference to Turnbull's work on the Pygmies, but refers instead to his studies of the Ik of Uganda whose behavior is more congruent with a sociobiological model.(18)

Similarly, Patricia Draper's account of the !Kung Bushmen of the Kalahari desert reveals that the women provide from 60 to 80 percent of the daily food supply. Their gathering requires them to go quite a distance from the camp: eight to ten miles is not unusual. Women and men are equally likely to be absent from the camp. The women are skilled in understanding the meaning of animal tracks and provide invaluable information for the male hunters. The women also have the "ability to discriminate among hundreds of edible and inedible species of plants at various stages in their life cycle."(19) Furthermore, men are in no sense the dominant authority figures. The women control the food they collect to a far greater extent than the men whose kill is divided according to a rigid set of rules.

This egalitarian situation is changing as the Kung are being resettled with agricultural people and their whole way of life is undermined. These changes are important to understand (even though we cannot provide that understanding here). Explanations in terms of genetic characteristics or of hormones do not enhance that understanding. In the agricultural situation, the men acquire certain kinds of responsibilities and chores that are not matched by those of the women, the mode of child rearing is changed, and so on. These are just two of the variables that are ignored by a biologically based form of analysis.(20)

Biological determinists do not deal with any of this data. While Wilson claims, for example, that data from the Bushmen support his view of human nature, he virtually ignores the material collected by Patricia Draper, and uses Richard Lee's findings very selectively. Lee is one of the world's foremost experts on the Bushmen and for all practical purposes Wilson ignores his work.

Why Do We Have Sociobiology?

While claiming to be scientific, then, Wilson, like the other biological determinists, makes no attempt to deal with material that does not support his theories. This is not science; it is propaganda, touted by the media not for its scientific merits but for its political functions.

Sociobiology's descriptions of what men and women do are wrong for the United States as well as for other societies. Wilson and those like him are attempting to convince people that women's place is really in the home. Yet women are an essential part of the labor force, one of the most exploited parts of the labor force in fact. For their exploitation to continue unchallenged, women and men must accept the idea that "women's work" is something secondary to their lives, something they should not really be doing.

Certainly some women spend most of their time in what Wilson calls the "residential area" but an ever increasing proportion do not and cannot. Only 24 percent of women were in the labor force in 1930, now 48 percent of women are working or looking for work.(21)

Capitalism needs women in the work force. Changes in the occupational structure with a lower proportion of industrial and manual jobs and a concomitant increase in office work has necessitated hiring people who are relatively educated but also willing to work for low wages. Women fit the bill. Women in 1960 were 97 percent of the secretaries, 84 percent of the bookkeepers, 96 percent of the telephone operators and 86 percent of the file clerks.(22) While the demand for women workers has increased, it has simultaneously become more of an economic necessity for even married women to work, to offset what would otherwise be a drop in their husband's real earnings.(22) In addition, the number of female-headed households has doubled between 1940 and 1975 and these women need to work as well.(23)

The work women do may be crucial but this is not reflected in their wages. According to Labor Department figures, the difference between an average man's

annual earnings and those of the average woman are now over $5,000. From 1955 to 1974 the gap between men's and women's earnings increased by an amazing 74 percent. The typical white man working year-round full-time in 1974 earned $12,343; the typical black man under those conditions earned $9,082. Year-round full-time white women workers brought home an average of $7,025, and wages for black women were $6611.(24)

The women's movement and its gains are under counterattack, including direct political repression. Under the Freedom of Information Act, the F.B.I. has released a 1377 page report which chronicles its anti-feminist activities between 1969 and 1973. The Church Committee of the U.S. Senate, in investigating government intelligence activities, produced evidence that the C.I.A., military intelligence and local police red squads have also operated against women's groups.(25)

These figures reveal in statistical fashion a major aspect of women's oppression. The women's liberation movement has attacked and is attacking the exploitation that women suffer. All agree, whatever else their differences, that the situation must be challenged and a new social order built. There is energy, strength and organization among women. Links have been built and continue to be forged with other political groups and struggles. Many women have developed a new confidence in themselves and their own abilities in fighting the myriad forms of sexism.

Theories of biological determinism are yet another kind of weapon used to preserve inequalities. Biological determinists such as Wilson have not consciously decided to protect American capitalism from the threat of women's liberation, but their ideas are used by the people who control the media, the publishing industry, and the scientific and social scientific establishments. Those who create these theories are rewarded: they are given money and prestige. Ambitious students and colleagues see which way the wind is blowing and add to the proliferation of books and articles that so misrepresent the real nature of human beings.

The faults in our society, the injustices, the inequalities do not lie in our genes; they are rooted in social institutions, and class structure. All over the world people have challenged — with a growing success — sexism, racism, poverty, degradation and brutality. Cuba, Vietnam, China, Mozambique, Angola are not utopias, but they are supporting a real effort to remove inequality. Whenever people join together to create a new order, those benefitting from the old try to crush the people and their vision. They use weapons to kill, maim and terrorize; and they use theories to demoralize people, to convince them of their essential inferiority, and to reconcile them to the world as it is. But despite the damage that they can cause, despite their capacity to hold back genuine progress, neither the bombs, the napalm, the armies, the C.I.A., nor theories of biological determinism can stop the movement to build a new society.□

References

1. Some examples of this are: Konrad Lorenz, *On Agression*, Harcourt, N.Y., 1966; Robert Ardrey, *African Genesis*, Dell, N.Y., 1967 (first pub. 1961); Desmond Morris, *The Naked Ape*, Dell, N.Y., 1969; Lionel Tiger and Robin Fox, *The Imperial Animal*, Dell, N.Y., 1971. Significantly, there was an article in the Oct. 1972 issue of *Fortune* Magazine, which called for more attention to be paid to our genetic propensities. See Tom Alexander, "The Social Engineers Retreat Under Fire".

2. Articles on Wilson's work have appeared in *Science for the People*, 7, 1975, No. 6, p. 28; 8, 1976, No. 2, p. 7; 8, 1976, No. 3, p. 33.

3. Edward O. Wilson, *Sociobiology: The New Synthesis*, Harvard Univ. Press, Cambridge, 1975, p. 55.

4. *The New York Times Magazine*, Oct. 12, 1975.

5. Eleanor Maccoby and Carol Jacklin, *The Psychology of Sex Differences*, Stanford Univ. Press, Stanford, 1974, p. vii.

6. Wilson, *New York Times Magazine*.

7. Leo E. Kreuz, and Robert M. Rose, "Assessment of Agressive Behavior and Plasma Testosterone in a Young Criminal Population", *Psychosomatic Medicine*, 34, 4, 1972, pp. 321-32.

8. For example, Jonathan L. Freedman, *Crowding and Behavior*, W.H. Freeman & Co., San Francisco, 1975, pp. 32-33.

9. Maccoby and Jacklin, p. 243.

10. David Pilbeam, "The Naked Ape: An Idea We Could Live Without", in David E. Hunter and Phillip Whitten, *Anthropology: Contemporary Perspectives*, Little, Brown & Co., 1975, pp. 66-75.

11. See for example, Jeffrey Rubin, Frank J. Provenzano, and Zella Luria, "The Eye of the Beholder: Parents' Views on Sex of Newborns," *American Journal of Orthopsychiatry*, 44, 4, 1974, pp. 512-519; Carol Seavey, Phyllis Katz, Sue Zalk, "Baby X, The Effects of Gender Labels on Adult Responses to Infants," *Sex Roles*, 1, 1975, 103-109.

12. John Nance, *The Gentle Tasaday*, Harcourt Brace Jovanovich, N.Y., 1975.

13. Colin M. Turnbull, *The Forest People*, Simon and Schuster, New York, 1962.

14. Ruth Sidel, *Women and Child Care in China*, Penguin, N.Y., 1973, p. 150.

15. Sidel, p. 114.

16. Turnbull, p. 110.

17. Turnbull, p. 154.

18. Wilson, *Sociobiology*, p. 544.

19. Patricia Draper, "!Kung Women: Contrasts in Sexual Egalitarianism in Foraging and Sedentary Contexts", in Rayha Reiter, ed., *Toward an Anthropology of Women*, Monthly Review Press, N.Y., 1975, pp. 77-109.

20. A useful book discussing the material basis for women's roles is Ernestine Friedl *Women and Men: An Anthropologist's View*, Holt, Rinehart and Winston, N.Y., 1975.

21. Arlene Eisen, "More Women Work — The Double Shift", *The Guardian*, Jan. 12, 1977.

22. Lise Vogel, *Women Workers: Some Basic Statistics*, New England Free Press, Somerville, n.d.

23. Eisen.

24. *The New York Times*, 11-29-76.

25. Arlene Eisen, "The FBI's Tactics Against Women", *The Guardian*, Feb. 23, 1977; Arlene Eisen, "FBI Targets Women's Movement", *The Guardian*, Feb. 16, 1973. See also Howard Husock, "The Feminist Papers: FBI Plays 'I Spy' " and "More Tales from the Feminist Papers", *Boston Phoenix*, Feb. 15 and 22, 1977.

Are Sex Roles Biologically Determined?

Freda Salzman

What Have Women Done?

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Acknowledgement: The analysis presented in this article is based in part on work done by the Women, Science and Social Control Collective. I am indebted to Marian Lowe for helpful discussions.

Re-emergence of "Biology is Destiny"

In the past ten years, a succession of highly publicized scientific works have purported to demonstrate that women's subordinate position in our society is due, in good part, to innate (genetic) differences between males and females, and not to external factors as claimed by the women's movement. These theories are simply new constructions of that old theme, "Biology is Destiny." Furthermore, these theories argue that sex roles are resistant to change and that dire consequences will result if we try to change them.

The current wave of theories about biological sex role differences took off in the mid-sixties with the beginning of a deluge of books of the "naked apery" genre.(1) By selectively using and misrepresenting the available data,(2) the proponents of the "naked apery" theory of human behavior claimed that male aggression, male dominance and war in our society were, and are, inevitable products of the genes which we share with the apes, the species evolutionarily closest to us. Other research reports seemed to confirm innate male aggressiveness by supposedly discovering that males with an additional Y chromosome commit violent crimes at a higher rate than do normal XY males. It was claimed that the Y chromosome contributes to aggressive tendencies in normal males, and that XYY "supermales" with two Y chromosomes are prone to "superviolence." (It is now known that there is no correlation of XYY males with higher rates of violent crime(3)—nor is there any credible vidence that normal males are innately more aggressive than females.(4))

In 1973, sociologist Steven Goldberg published *The Inevitability of Patriarchy*, which directly attacked the women's movement. The title of the book speaks for itself. Using highly questionable evidence,(4) Goldberg claimed that the subordination of women is an inevitable outcome of hormonal effects on males, which produce male aggression, genius and ability and which leads to male dominance. Finally, the attack on the women's movement — and on people in general who are seeking social change — escalated to a new and dangerous level with the publication in 1975 of E.O. Wilson's *Sociobiology: The New Synthesis*, a book which received the greatest acclaim of all in the current wave.

Sociobiology attempts to show that salient features of our present society are biologically determined. According to the theory, socially significant human traits are genetically based and have evolved under the influence of natural selection. Employing the same discredited methodology as the "naked apery" theory,(5) *Sociobiology* has at its heart the thesis that hierarchy and patriarchy are inevitable. The growing number of popularizations of the theory have particularly focused upon claims for innate male-female differences. The re-emergence of interest in sex-difference research is part of a general resurgence of biological determinist theories which try to demonstrate that our highly stratified society, based mainly on class, race and sex, is due to genetic differences between these groups and not to societal factors. Historically, biological determinism as a theory of the status quo has been widely publicized and encouraged during periods of considerable social unrest and questioning of societal institutions.

Clearly, the group which benefits most from such theories is an extremely small but wealthy and privileged class. This elite, through its extensive control of the media, education and funding institutions, can strongly influence public opinion and the direction of research. The elite finds it far easier to promote theories which undermine demands for equality than to spend money to eliminate inequality. For example, the main way in which women are discriminated against is through sex-stereotyped jobs. Women now comprise about forty percent of the wage labor force and are demanding equal pay for equal work. From the corporate managers' point of view, it would be excessively expensive to create that many new well-paying jobs.

Sociobiology is clearly comparable, in principles, scope and framework, to Social Darwinism, the most widely held theory of biological determinism in the later nineteenth and early twentieth centuries. Social Darwinism, which applied the ideas of evolutionary biology to social development, strongly justified the established social order of the period — a fiercely and brutally competitive capitalist economic system and a stratified social structure, again based primarily on class, race and sex. In the U.S. Social Darwinism provided important support for the eugenics movement which led to sterilization laws in a number of states and the immigration law of 1924, which restricted immigration on the basis of national origin.

In the remainder of this article, I will focus on the study of the sexual differentiation of the brain, for two reasons. One is that the mechanism currently proposed by scientists to explain sex-differentiated behavior is the effect of sex hormones on the brain. Since this research is fairly sophisticated and sounds credible, I think we should know more about this work, the questions it asks, and the evidence it offers. Second, I hope to show that the attempt to establish a genetic basis for social-sexual differences (or for inequalities based on class and race) in a society in which the sexes are differentially reared is "bad" science. Moreover, even if we could establish a genetic basis for sex differences in our society, this would tell us nothing about how plastic or resistant these traits would be to changes in the environment. The same criticisms have been leveled against attempts to establish a genetic basis for class and race differences. (This has been discussed with respect to the I.Q. controversy.)

"Sex in Brain"— Past

In *The Study of Sociology* (1873), Herbert Spencer ascribed women's inferior mental capabilities to their "arrested" evolutionary development, which was required in order to save their "vital power to meet the cost of reproduction." Furthermore, through "struggles for existence" in which the fittest women were those who survived against "aggressive, unscrupulous, intensely egoistic" stronger males, "women's nature" evolved to please men, to disguise their own feelings, to rely on intuition, and to admire male power — and power in general. These supposedly innate feminine attributes obviously reflected the upper- and middle-class Victorian ideal of true Womanhood.

Sociobiology has at its heart the thesis that hierarchy and patriarchy are inevitable.

Scientists attempted to "verify" women's mental inferiority and inferior "nature" by studying the size, weight, shape and fissural patterns of the brain. In 1887, a well-known New York neurologist and former United States Surgeon General claimed in an article in the *Popular Science Monthly* that the female brain was measurably inferior to that of a man. Incensed, Helen Hamilton Gardener (pen name of Alice Chenoweth Day, a prominent women's suffrage leader)(6) took up the study of brain anatomy and in 1888 wrote a paper entitled, "Sex in Brain." Assuming the data to be as alleged, she set out to prove that the brain mass of a woman was not demonstrably different from that of a man who grew up under the same conditions and with the same opportunity for development. As a result of her studies, she discovered that on the average, brain differences between persons of the same sex and achievements were much more marked than any that were known to exist between the sexes. Gardener willed her own brain to the famous Wilder Brain Collection at Cornell University. Examination of her brain was reported to reveal

high development in all regions and supposedly fewer sex differences than in other female brains. At the time, this was attributed to her being an exceptional woman.(7) The general effort, however, to correlate gross brain structure with intellectual achievements and personality traits was unsuccessful, and interest in this work tapered off.

At first women perversely came out ahead on what were considered to be crucial measurements: ratios of brain weight to body weight(8) and scores on the first Stanford-Binet IQ test in 1916.(9) These results were quietly put aside. Subsequent IQ tests were standardized so that males and females had equal averages.

Freud's theory of the psychology of women finally located a woman's crucial deficiency — she lacked a penis. From this indisuptable anatomical fact, Freud constructed a whole theory — now known to be without substance(10) — which explained women's subordinate role in society, a theory based on the biological determinist notion that "Anatomy is Destiny." The selective use of Freudian ideas in this country helped to reinforce woman's traditional — and subordinate — role as wife and mother and to provide an ideology by which the increasing number of women who were entering the wage labor force failed to see themselves as real workers. Due to the predominant belief that the differential social and economic status of different groups in society was biologically determined, Freudian ideas were readily accepted *even by women.*

Current Research

There has been considerable effort in current research to demonstrate that supposed innate differences in male-female behavior are due to the effects of sex hormones on the brain. Extensive studies in rats have shown that in a critical period during the first week after birth, the testes of males secrete the male hormone testosterone which organizes or, as it descriptively put, "lays down the circuits for," a male brain. The male-organized brain is irreversible, leads to an essentially acyclic (male) pattern of sex hormone production, and determines characteristic stereotyped male behavior, which in rats is mounting and fighting. The brain of a rat which is not exposed to testosterone (or high levels of other sex hormones which are closely related) during the critical period develops an irreversible female-organized brain. This leads to a cyclic pattern of ovarian function and ovulation, with an estrus period ("heat"), when stereotypical female behavior is displayed. This behavior includes assuming the lordosis position (elevation of the rump) in the presence of mature males.

The part of the brain of the rat which is most involved in this sex-differentiated behavior is the hypothalamus and the limbic system (an "old" or "primitive" part of the mammalian brain in terms of evolutionary development)." The hypothalamus-limbic system is concerned with the self-regulation of the various body functions such as eating, drinking, and mating behavior, as well as emotion and motivation. The hypothalamus both secretes and responds to hormones. The hypothalamus of the female rat has been shown to regulate, through its effect on the pituitary and the feedback influence of ovarian hormones, the cyclic pattern of ovulation. This mechanism appears to be the basic one for female cyclicity in all mammalian species.

Scientists are now trying to explain the differences in behavior between men and women by the effects before birth of sex hormones on the brain.

What do these results mean for human behavior? Biological determinists claim that the hypothalamus-limbic system is "wired" or "programmed" for instinctual behavior, as Wilson does in the opening paragraphs of *Sociobiology:*

> The hypothalamus-limbic complex of a highly social species such as man [*sic*], "knows," or more precisely it has been programmed to perform as if it knows, that its underlying genes will be proliferated maximally only if it orchestrates behavioral responses that bring into play an efficient mixture of personal survival, reproduction, and altruism.(12)

Other biological determinists are less "deterministic" with respect to the role of sex hormones in establishing male-female differences. They realize that the diversity among individuals of one sex is greater than the average difference between males and females. Furthermore, there is tremendous cross-cultural variation concerning what is considered "male" and "female" behavior. These "limited" determinists acknowledge that the predominant influence in establishing gender differences is the social transmission of behavioral norms. Money and Ehrhardt, in their book *Man and Woman, Boy and Girl*, describe cases in which chromosomal XX females were successfully reared as males and chromosomal XY males as females. These cases indicate the overriding importance of social factors. Nevertheless, on the basis of sex hormone studies in animals and extremely limited studies of humans, Money and Ehrhardt emphasize throughout the book the effect of prenatal hormones in organizing male-female brains. They state that the presence or absence of male hormones establishes "certain patterns or organization in the brain, especially, by inference, in the hypothalamic pathways that will subsequently influence certain aspects of sexual behavior."(13) They claim that chromosomal females exposed

to excess prenatal androgens, the male hormones, are prone to be "tomboys," while chromosomal males exposed to insufficient prenatal androgens are prone to be "rather placid."(14) T' is theme is rapidly being picked up by others, who then argue that inequality of the sexes is due in good part to biological differences arising from hormonally produced, sexually differentiated propensities in behavior.(15) Thus, for example, anthropologist Patricia Draper speaks of the importance of fetal hormones in "pre-programming" the brain which causes the sexes to respond differentially to certain stimuli. Sociologist Alice Rossi adds a "bio-evolutionary perspective" to show how sexually-differentiated behavior evolved under the influence of natural selection, a purely speculative discussion once again made respectable by sociobiology.

What have actual studies in humans demonstrated? Needless to say, human females have not been observed to go into estrus during which time they present their rumps to mature males. In humans and in non-human primates, there does *not* seem to be a critical period for the irreversible differentiation of the brain with respect to the male or female pattern of hormonal production,(16) although some sex-differentiation is expected in the hypothalamus. The cyclic female pattern can occur after human (and non-human primate) females have been exposed to high levels of androgens before and after birth, and experiments on rhesus monkeys indicate the capacity for cyclical response remains totally intact in adult males. So even this "old" part of the brain, which one might expect to be the most "wired" or programmed with respect to this specific physiological function, appears to be far more plastic in humans — and non-human primates — than originally anticipated, and also *not* to be sexually differentiated.

The hypothalamus appears *not* to be sexually differentiated in humans and so is incapable of explaining sex differences in behavior.

In order to explore the effects of hormones on behavior, studies of the behavior of women with respect to hormonal change during the menstrual cycle have been carried out. These studies — clearly not done to help women — seek evidence of a "wired-in" response, that is, correlations between mood, sexuality and mating positions, and hormonal levels. At best we can say that these studies are inconclusive and provide evidence of the methodological problems with all such investigations. Hormones cause real, noticeable physiological changes, as women using birth control pills have found

Photo by Ellen Shub

out. The so-called premenstrual syndrome experienced by many women and characterized by heightened irritability and tensions may be due to physiological factors such as water retention, constipation, nausea and breast pains.(18) In particular, depression and water retention may be caused by sodium retention and potassium depletion in cells, due to the sharply dropping levels of progesterone (one of the hormones produced in the cycle) prior to the onset of the period. Inadequate diet and insufficient excercise may also contribute to the physiology of the syndrome. Socialization appears to be an important factor since a woman's attitude towards her oncoming period may influence her mood. In addition, the very definition of the premenstrual syndrome appears to be questionable.(19)

One of the most frequently cited human examples which is claimed to demonstrate the sexual differentiation of the brain (and which supports the contention that the results found in rats also apply to humans) is that of a small number of chromosomal XX females who were exposed while still in the womb to unusually high levels of androgens, the "male" hormone. These fetally androgenized females, as they are called, were actually born with male genitalia in various stages of development and later underwent corrective surgery. The subsequent behavior of a group of twenty-five fetally androgenized females, along with that of a control group, was studied by Money and colleagues and reported in *Man and Woman, Boy and Girl*. These fetally androgenized females were reported to engage in more "tomboyish" activities than the controls, but not to

such an extent as to make them conspicuously different from other girls. This whole effect can be explained plausibly by environmental differences, resulting from the parents' ambivalence toward their daughters born with male genitalia. Some of the females, in fact, were actually assigned the male sex at birth.

It is now clear that in humans and non-human primates, the cerebral cortex (the "new" brain) tends to dominate brain functions, and modifies the influence of the hypothalamus-limbic system on the expression and regulation of emotion. In humans, the cerebral cortex has primary control of all higher functions and is extremely plastic. A given physiological state in humans can have a variety of quite diverse behavioral expressions, depending on past learning experience and the social situation.(20)

The latest claims for a sex-differentiated brain are based on psychological tests of functions which involve the cerebral cortex. The brain is divided into two hemispheres, and a number of functions appear to be controlled in the left or right hemisphere. This is called left or right lateralization. Language and analytic abilities appear to be left lateralized; visual-spacial functions to be right lateralized. There are some reports that boys' and girls' brains may be lateralized differently with respect to certain tasks such as those involving visual-spacial ability. The hypothesis is now being put forward that these differences in lateralization are correlated with differences in performance.(21) This suggestion has been seized upon to explain women's supposedly inferior mental achievements — and, as usual, has gotten front-page publicity in the newspapers, as in an article entitled "Why there are few female geniuses," which appeared recently in a Boston newspaper.(22)

Rats from the same litter placed in different environments have measurable differences in brain anatomy and chemistry. Such a fact makes it difficult to attribute even differences in brain anatomy to genetic factors.

Differences in lateralization in boys' and girls' brains (if these results are correct) bring us back to the claims being made in the 1880's of innate male-female differences in gross brain anatomy and their correlation with intellectual achievement and personality traits. Helen Hamilton Gardener's objection to these claims is still valid today and is supported by other experiments which show that structural and functional differences in the brain may result from environmental influences.

General Critique

What conclusions can we draw from these studies? My various remarks give some of the basis for the statement, that there is at this time no credible scientific evidence that genetic basis exists for *any* socially significant human behavior trait (in the normal range) or for social forms in our society — and, of course, this includes male-female behavioral differences and sex-roles in our society. Claims to date of genetic bases for socially significant traits are either methodologically flawed or purely speculative.

It is simply not possible to divide behavior into separate genetic and environmental components and to discuss them separately.

One might still ask: What about sex-differentiation of the brain? As I indicated above, on the basis of experiments on rodents, it has been anticipated that there will be some biological differences between male and female brains at the level of the hypothalamus, although less than once believed. If differences in lateralization in boys' and girls' brains exist, they may well be accompanied by measurable differences in the brain anatomy. (Parenthetically, I must add at this point that differences in the average performance between any groups based on race, class or sex, are usually small compared to differences between individuals of a given group. Thus, there is usually considerably overlap in abilities. It is rather manipulative use of statistics to explain, for example, sex-roles as being due to average differences between males and females, since in almost all traits there are a good number of females who score higher than most males and this great diversity is not at all reflected by the positions which males and females have in our society.)

The question, then, is if biological differences exist, what is their significance? That is, to what extent are the differences due to genetic or to environmental determinants? In effect, we are back at the Nature-Nurture question which re-emerged with the claim made by Jensen and Herrnstein that the difference in mean IQ scores between races and classes was due to genetic factors. Some of the crucial data, that of Cyril Burt, on which Jensen and Herrnstein based their argument, has been shown to be fraudulent. But even if this were not the case, careful critiques show that the fundamental question which Jensen and Herrnstein raised is *strictly meaningless scientifically*.(23) It is simply not possible to divide behavior into separate genetic and environmental components and to discuss them separately.

The reason for this is that there is an extremely important part, called the genetic-environmental interaction, which makes such a division mathematically impossible. The following hypothetical example illustrates this point: Organism A with a particular set of genes (called genotype) performs better than organism B with a different genotype on some task in a particular environment. Let us assume that 80 percent of this difference is due to genetic factors. This does *not* mean that in every conceivable environment A will perform better than B. In another environment, A may well perform worse than B. In humans, *many* genes are expected to contribute to the expression of any *single* trait. We have no basis for assuming that the performance of all genotypes will vary in the same way with environment. This means that even if we could establish that the difference between two groups in the performance of a particular task was partially due to genetic differences, this result would only be meaningful with respect to the particular environment studied. It would tell us nothing about how either group would perform in another environment.

Reports that boys' and girls' brains may be lateralized differently have been seized upon to explain women's supposedly inferior achievements.

We now come to the more immediate methodological problem in the nature-nurture question which we posed earlier: What can we say about biological differences if they exist between two groups in our society? This methodological problem is illustrated by studies of rats conducted by M. Rosenzweig and colleagues and described in an article "Brain Changes In Response to Experience."(24) Their experiments demonstrate that rats from the same litter, who are placed in environments which provide different experiences and stimulation, show measurable differences in brain anatomy and chemistry. The experimenters find, for example, that the rats in the "enriched" stimulative environment have thicker cerebral cortices than those from the "impoverished" environment. There are other changes as well, down to structural differences at the synaptic junctions where nerve cells make contact.

The conclusion is that it is impossible to say anything about the genetic basis for differences observed between any two groups raised in different environments. Discovery of biological differences in brain function or differences in performance between two groups reared in different environments cannot be attributed to genetic differences between groups. There is, moreover, a good body of evidence that indicates that environments for boys and girls differ from the time that social interactions begin following birth.

Just as in the case of the supposedly more biologically determinist theories which claim that differences in socially significant behavioral traits are genetically based, the suggestions that the fetal hormones organize the brain into male-female patterns which predispose the sexes to have certain "tendencies" or make them differentially sensitive to certain stimuli cannot be tested for humans. Under these circumstances, any attempt to perform these experiments is "bad" science. Moreover, all the observations for which this hypothesis has been invoked to explain can quite plausibly be explained by environmental factors, as critics of Money and Ehrhardt(4) and Patricia Draper(15) have pointed out.

Finally, we see that studying the genetic basis of human traits in any given environment tells us nothing about what the trait would be like in another environment — particularly, how plastic or resistant a trait will be to changes in the environment. We lack a general theory to make any predictions. Even though we do not have a general theory, however, there is very good evidence that we can proceed empirically to try to effect social change. In certain third world countries, the position of women has altered radically in the last few decades: in China, Cuba, and Vietnam, the position of women has improved dramatically; in parts of Africa, which are undergoing further penetration and development under the influence of Western capitalism, women's position has deteriorated.(25) Such rapid changes, some for the better and some for the worse, are difficult to explain by biological determinism, since genetic changes are extremely slow. These changes are clearly correlated with rapid economic, political and social changes. We have every reason to believe that the creation of a just and equal society is *humanly* possible.□

REFERENCES

1. R. Ardrey, *African Genesis* (Dell, 1961), *The Territorial Imperative* (Dell, 1966) and *The Social Contract*; K. Lorentz, *On Aggression* (Bantam, 1966); D. Morris, *The Naked Ape* (Dell, 1967) and *The Human Zoo* (Dell, 1969); L. Tiger, *Men in Groups* (Vintage, 1970); and L. Tiger and R. Fox, *The Imperial Animal* (Dell, 1971).

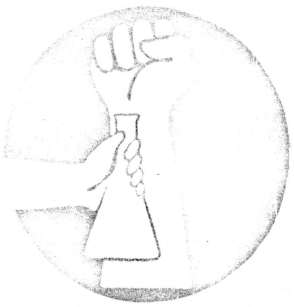
2. A. Montagu, ed., *Man and Aggression* (Oxford, 1968); D. Pilbeam, "The Fashionable View of Man as a Naked Ape," in *New York Times Magazine*, September 3, 1972, p.11; S. Slocum, "Woman the Gatherer: Male Bias in Anthropology," in *Toward and Anthropology of Women*, ed. R. Reiter (Monthly Review Press, 1975), p.36; and R.R. Leavitt, *Peacable Primates and Gentle People* (Harper and Row, 1975).

3. H. Witkin, *et al.*, "Criminality in XYY and XXY Men," *Science*, 193 (13 August 1976), p.547.

4. R. Bleier, "Myths of the Biological Inferiority of Women," in *The University of Michigan Papers in Women's Studies*, Vol. II (1976), p. 39.

5. B. Chasin, "Sociobiology: A Sexist Synthesis" in *Science for the People*, 9 (May-June 1977), p. 27.

SEX ROLES, *continued on p. 43*



Science for the People

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THE POLITICAL USES OF SOCIOBIOLOGY IN THE UNITED STATES AND EUROPE

by

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If you happened to see the Feb. 2, 1980, issue of Saturday Review, you might have noticed an article entitled "Le Nouveau Nazism - A Rightist Fashion from France."[1] The article quotes August von Wageneck, a correspondent for the German newspaper Die Welt:

> "When I leaf through Alain de Benoist's magisterial work View from the Right, I, a German, find once more the distant nightmares of my youth... I heard those same questions being posed 40 years ago during the Schulungs-Abende, the indoctrination evenings of the Hitler youth in my native Rhineland."

What does this have to do with an academic field of research called sociobiology, which has concentrated on studies of animal behavior? De Benoist is a leading theoretician of la Nouvelle Droite (New Right) in France, an extreme right-wing but influential group which relies heavily on academic theories of genetics and behavior, including sociobiology, to support their ideology. How did this rapid progress of an academic discipline into a political tool come about in the five years since the widespread publicity surrounding the appearance of Professor E.O. Wilson's Sociobiology: The New Synthesis?[2] This article will trace this course of events and describe the political applications of these theories in both the United States and Europe.

When groups have attempted to gain power in a society in order to implement a political program which was hierarchical, racist and sexist, they have often relied on ideological support from religion, philosophy or science. Similarly, whenever the status quo in a society is threatened by movements of people demanding changes in social arrangements, the same sources of ideological support have often been called upon.[3] Whether intentionally or not, academics have consistently come up with rationales for existing social structures when they were called for. Recently, science has assumed a major role in this process of ideological justification. If political ideology can be cloaked in the supposedly objective language of science, then it has all the more power. I believe that the natural sciences have little to tell us about how we should structure our societies and that sociobiological theories and others of that class on the biological basis of complex human social behavior can make no meaningful statements about complex societal problems. I would suggest that these theories do not arise, in the main, because of any new scientific information, but rather appear in response to particular social conditions.

Specifically, in the U.S., a resurgence in the use of science in this way, the biologicizing of social problems, began around 1969. These arguments followed the period of turmoil of the '60's. During this era, movements of black people, other minorities, the poor and women, and generally increased radical political activity,

led to strong demands for greater equality of wealth and power in this society. In 1969, Arthur Jensen published his famous article in the Harvard Educational Review suggesting that the demands of the '60's by blacks for equality were unachievable since blacks were genetically inferior in intelligence to whites.[4] This was followed in 1971 by Harvard Prof. R. Herrnsteins' suggestion that radical demands for equality were unrealizable since classes were genetically determined.[5]

Then, in 1975, came E.O. Wilson's book. The media widely spread the claims of human sociobiologists that the following social behavioral patterns had a large genetic component; male dominance over females; hierarchies and classes; and racial antagonism. Of course, the sociobiologists often included qualifiers to the implications of these conclusions, but just as often they went on to publicly suggest implications for social policy.

The Sociobiology Study Group of Science for the People first met in 1975 in response to the pre-publicity accorded Wilson's book. We were quite concerned about the uses to which such ideas would be put as well as being aware of the lack of scientific foundation for them. We published that year a letter in The New York Review with this perspective. Speaking of the eugenics movement in the earlier part of the 20th century, we said:

> "These theories provided an important basis for the enactment of sterilization laws and restrictive immigration laws by the United States between 1910 and 1930 and also for the eugenics policies which led to the establishment of gas chambers in Nazi Germany. The latest attempt to reinvigorate these tired theories comes with the alleged creation of a new discipline, sociobiology."[6]

Wilson responded to this in a subsequent letter in The New York Review stating:

> "I resent this ugly, irresponsible, and totally false accusation."[7]

What I would like to describe here is the realization of some of the fears presented in that first letter: the use of human sociobiological ideas to support the ideology of neo-fascist groups in Western Europe, notably England and France. To understand this phenomenon, it is important first to look at the reception of sociobiology in the United States since 1975.

Sociobiology in the United States

In the United States, sociobiology has been widely and popularly received. Its ideas have been cited frequently in defense of the status quo - that is, against calls for social change. This popularity can be traced back to 1975 when Harvard University Press and E.O. Wilson garnered enormous publicity around the publication of Wilson's book. This began with a front page New York Times article announcing the publication of the book and stating its revolutionary implications. This was followed by TV interviews,

interviews in magazines such as House and Garden, People, OMNI, Psychology Today and innumerable other popular accounts.[8] The media coverage has continued almost unabated since. The articles have mentioned little of the animal work but have focussed on implications of sociobiology for human social arrangements. The claims in the popular media for the implications of sociobiology cannot be considered distortions of sociobiological theory and, in fact, have often been praised by sociobiologists. As we will see, these claims can be traced directly to proposals put forth in the serious academic work of certain sociobiologists.

A major public focus of sociobiological theory in the United States has been on the question of sex roles - i.e. why are men and women for the most part fulfilling different roles in the society? But, in addition, a number of other social issues have been dealt with by sociobiological reasoning. For example, Time Magazine published a cover story entitled "Why You Do What You Do."[9] Included in the article were statements such as "the theory can explain loyalty to church, corporation and nation" and a claim by Robert Trivers that feminism was going to decline because feminists were not marrying and having children and, therefore, would not pass on their feminist genes!

The Time article ends with an important point "Sociobiology may have appeared at the right cultural movment. The 1970's have brought with them growing impatience and disillusionment over failed educational and environmental experiments designed to alter social behavior. At such a time, the emergence of a doctrine preaching that man is caught in history, able to exercise free will only within the limits set by his genes, may do very well indeed." This emphasizes a point we have made repeatedly: that the reception sociobiology has received in the USA has more to do with the social context into which it has been introduced than with any new scientific information.

An article in Business Week entitled "A Genetic Defense of the Free Market"[10] includes the following:

> "Self-interest is the driving force in the economy because it is ingrained in each individuals' genes." "Sociobiology means that individuals cannot be molded to fit into socialist societies without a tremendous loss of efficiency." "Bioeconomists say that government programs that force individuals to be less competitive and selfish than they are genetically programmed to be are preordained to fail."

These statements are made by academic "bioeconomists" who derive their theories from sociobiology, which emphasizes "The Selfish Gene." Wilson, himself, has made categorical statements about possible social arrangements. In "On Human Nature", he states that the societies proposed by William Graham Summer, The Social Darwinist, and Mikhail Bukhanin, the anarchist, are "biologically impossible."

With regard to sex roles, Wilson has stated that our evolutionary heritage has resulted in a situation in which men are able to achieve more in business, science and politics. If we want to change this situation and create greater equality between the sexes, according to Wilson, we will "place some personal

freedoms in jeopardy."[11] According to sociobiologist David Barash "Sociobiology explains why women have almost universally found themselves relegated to the nursery while men derive their greatest satisfaction from their jobs."[12]

In an article in Playboy magazine on Sociobiology, with the headline "Do Men Need to Cheat on their Women? A New Science Says Yes," the following claims are made "Males are also driven by their gene to reproduce. They tend to be more promiscuous because in time past that was their best way to reproduce the most offspring. If you get caught fooling around, don't say the devil made you do it. It's the devil in your DNA." "Females should not be too hasty in criticizing men for being caught up in political games or for being obsessed with wealth and power. After all, those are the very criteria by which females have been choosing men all these eons, to better their own genetic interests." "A female is probably less threatened by her mate's adultery than he is by hers. The two sexes seem to differ in the amount of infidelity they will tolerate in their partners."[13] This article provides legitimization of the double standard and of a variety of forms which male dominance over females takes. But it is not a misrepresentation of sociobiology, for the ideas are taken directly from the arguments of leading academics.

A key feature of sociobiology is kin selection. According to this theory, individuals will favor (e.g. sacrifice themselves for) those who share the most genes with them. A number of interesting examples have been reported from animal studies which are consistent with this model. Trivers, Wilson and others have extended the concept without any supporting evidence to explain many aspects of human social behavior, including altruism and racial prejudice.

> "Human racial prejudice, directed against individuals that look different could have its roots in this tendency to distinguish in-group from out-group."[14]
>
> "(racial) antagonisms are more or less built into the species. The only sound solution lies in miscegenation."[15]

This ascription of racist attitudes to genetic proclivities could easily be used in this society to distract attention from the historical and economic roots of our current racial quagmire. As I shall point out, these implications of sociobiology have been appropriated in Europe, but in the United States have received less attention. This may be, in part, due to the strong reaction which Jensen's genetic theories of racial inequality engendered here and the resultant lack of respectability for biological theories relating to race problems.

For other examples of the ways in which sociobiology has been promoted publicly as an explanation for human social arrangements and social problems, the reader is referred to reference 8. The result of this barrage of publicity for sociobiology has been a legimitization for a theory which has more recently been appropriated by neo-fascist and right-wing groups in Europe.

Sociobiology in Europe

There exists in Western Europe a network of neo-fascist groups which vary in their strength and political influence.[16] They publish several journals which attempt to provide academic respectability. These are Nouvelle Ecole in France, Mankind Quarterly in England and Neue Anthropologie in Germany.

Among the board of sponsors of Nouvelle Ecole are Robert Ardrey, Konrad Lorenz, Carleton Coon (who published books on the discredited theory that blacks and whites are the products of separate evolution) and Arthur Jensen. Jensen is also an honorary adviser to Mankind Quarterly and publishes articles in Neue Anthropologie. There are close connections between these journals with common members of editorial boards and sponsors, and reprinting of articles from one journal in another. A number of the leaders of these movements are connected with earlier Nazi race theorizing or with post-WWII neo-Nazi groups. The pages of their journals are filled with praises of the race theorists of Nazi Germany including Hans Gunter and Eugene Fischer. The articles are often virulently racist, anti-semitic and anti-feminist and call for a new elitist and hierarchical society.

The most politically influential of these groups is in England. The journal, Mankind Quarterly has articles by former Nazis and promotes claims that 6 million Jews were not killed by Nazi Germany. The political face of this movement is the National Front, the successor to Oswald Moseley's British Union of Fascists, a group sympathetic to the Nazis. The National Front has had significant impact on British political life. Their success several years ago in local electoral campaigns is said to have been a major factor in the shift to the right in the British Conservative party. The nomination of Margaret Thatcher and her subsequent racist statements in her campaign were probably an effort to direct votes away from the growing strength of the National Front. The success of the National Front can be seen at least in part to be due to the steadily worsening British economy coupled with an increased black and Pakistani immigrant population. Thatcher has recently proposed an immigration policy which would specifically reduce immigration of people of color.[17] In the last couple of years, the direct influence of the National Front has declined, in the face of strong reactions from progressive forces and as a result of the appropriation of many of their policies by the current government.

The National Front, the Mankind Quarterly and other similar groups and publications have consistently used the ideas of Arthur Jensen and Hans Eysenck (the English counterpart of Jensen) to support their politics. One of many examples is a leaflet handed out to school children all over England which includes this section: "The Racial Equality Lie - Tell the Red Teacher that Top Scientists like Jensen and Eysenck Say this is Rubbish." The major publication of the National Front is the monthly Spearhead. In an article published there in 1973, Marvin Webster their National Activities Organizer stated "The most important factor in the build-up of self confidence among racists and the collapse of morale among multi-racialists was the publication in 1969 by Arthur Jensen in

the Harvard Educational Review." The magazine is full of articles justifying racism and anti-semitic attacks on critics of IQ-genetic studies. For instance, it is pointed out that Ashley Montague's real name is Israel Ehrenburg. Or, beside a picture of Jewish Professor Steven Rose, a leading critic of IQ-genetic studies: "One glance at Prof. Rose's photograph is enough to see why he would like the concept of racial equality to be as chaotic and indeterminate as his own." Articles praise South Africa's Apartheid system and call for continued white rule in Zimbabwe (Rhodesia). There are also anti-feminist articles including one by Alain de Benoist.

Most recently this right-wing movement has promoted Sociobiology. In the March, 1979 Spearhead appeared an article by Richard Verrall entitled "Sociobiology: The Instinct in Our Genes." It included the following statements:

> "The pioneers of this school, which has sometimes been described as anthroposociology have of course been scorned as antiquated 19th century racists whose backward ideas paved the way for Adolph Hitler. Today, however, the wheel has turned full circle to vindicate the anthroposociologists. The theory that behavior and social organization are determined to a crucial extent by genetic inheritance is now central to that most progressive branch of the biological sciences called sociobiology." "The study of this subject is now being pursued by sociobiologists led by Harvard zoologist E.O. Wilson." "Our racialist viewpoint which sees the national family as an organic whole within which the individual fulfills his wider purpose of contributing to its strength and survival, is endorsed by the sociobiologists.

The article also uses sociobiology to condemn feminism as Marxist rubbish and to conclude that social classes are genetically determined.

In France, a growing political force has been recently accorded wide publicity and given the name Nouvelle Droite or New Right.[18] The recent successes of this group can be attributed to a number of factors. These include the highest unemployment probably since before World War II coupled with large numbers of immigrants from Southern Europe and North Africa. This right-wing movement in France includes members of the current government. Its most influential member is Michel Poniatowski, right-hand man of Giscard d'Estaing, the President, and a former Interior Minister.

The editor of Nouvelle Ecole is Alain de Benoist a former neo-Nazi. De Benoist claims to be the first to publicize Jensen in France. An interview with Jensen was published in this journal as were certain of Jensen's articles. The journal includes other articles on race and eugenics. While the views presented are extreme and often reminiscent of Nazi theorists, they have enough respectability in France that de Benoist received the coveted Grand Prix of the Academic Française for his book Vu du Droite (View from the Right) (see introduction to this article). The New Right has recently taken control of Le Figaro Magazine, which

has a circulation of 450,000. This magazine is put out by the publisher of Le Figaro newspaper, Robert Hersant, himself a former Nazi collaborator.

As in England, Sociobiology has been discovered by the New Right.[19,20] Several American journals have noted this.

> "A vigorous group of right-wing thinkers is now challenging the left's long standing intellectual hegemony, proclaiming ominous theories on race, genetics and inequality rarely heard since the dark days of the third Reich." "New Right partisans hold that individuals and races are divided by insurmountable barriers of hereditary inequality. In support of this view, they cite the much debated research of such American scientists as Arthur Jensen, William Shockley and Edward O. Wilson."[21]

Louis Pauwels, editor of Le Figaro Magazine, is quoted in the New York Times: "The life sciences reveal the necessity of a hierarchy of individuals in all advanced animal societies as in human societies."[22]

And in a recent N.Y. Times article on the murder of a Jewish left-wing leader

> "The two incidents (a murder and bombing) followed a series of debates on television and in the press about the position of Jews in present-day France and about hidden antisemitism. The issue has been brought before the public by the emergence of a group of intellectuals calling themselves the "new right" who argue that there is a scientific basis for elitism in 'socio-biology.' (sic)"[23]

The appearance of this group has stimulated a political debate in France in newspapers and on TV. Alain de Benoist published a major article in Le Figaro Magazine on Sociobiology.

> "The laws of life make equality impossible. This is the revolutionary message of 300 of the greatest English and American scientists: the sociobiologists."[24]

The article also argues against feminism.

There has been a reaction in France from center to moderate left newspapers and magazines. These include Le Nouvel Observateur which states:

> "Next to Indo-European Archaisms, the strongest of the cultural anchorages of the new right is biology. For a long time, in fact, the right has not produced a coherent theory to support hierarchy. What is important is to find some arguments to legitimate its political discourse on hierarchy."[28]

Sociobiology has provided that legitimization. The magazine Le Point and Le Monde have published scientific critiques of Sociobiology.[26,27,28]

Interestingly, in France, people coming from different political perspectives seem to be much more sensitive to the role and implications of sociobiology, perhaps because of the much closer contact with events in Germany earlier in this century. For instance, Yvon Blot, a member of a New Right study group, Club d' Horloge, stated "Sociobiology is making spectacular progress. It cannot be ignored just because it is close to certain Nazi themes."[29]

Most recently, E.O. Wilson granted an interview to Le Figaro Magazine which accompanied an article entitled "Confirmation: l' intelligence est hereditaire" (Confirmation: intelligence is hereditary). The interviewer was Yves Christen, author of the book, L'Heure de la Sociobiologie (Albin Michel, Paris). In the interview Wilson states, without qualification:"It has clearly been established that intelligence is, for the most part, inherited." This statement is, in a scientific sense, meaningless, since the evidence on heritability of intelligence has been discredited. Further, by implication, it suggests a fixity to the quality of intelligence (whatever that is) which is at odds with genetic theory. That is, the heritability of a trait only has meaning in a particular environment. Even if there were evidence that, under present social and educational conditions, intelligence had strong heritability, that evidence would tell us nothing about how much intelligence might be improved by altering those conditions. However, the interview is not meaningless in a political sense. By granting such an interview to a group whose political goals are so reminiscent of the Nazis, and in the context of an article which claims scientific justification for its ideology, Wilson, unwittingly or not, has lent his prestige to this movement.

Conclusion

In summary, I believe that science, in general, and sociobiology in particular are not neutral pursuits free of the surrounding social and political influences. The appearance and the popularity of sociobiology are intimately connected with social conditions of the '60's and '70's. Those attempting to construct a science of human behavior, whatever it is, must inevitably incorporate social assumption, into the theory.[8] Those assumptions give direction to the theory and lead to a particular view of human social institutions.

I am not saying that sociobiology can create race prejudice or sexism, or that the debunking of sociobiology will eliminate these problems. However, it is clear that both historically and today groups who have used these ideas consider them powerful tools to rationalize their policies. In this sense, the attempt to expose the scientific flaws and the political aspects of sociobiology is an important component of the struggles against racism and sexism in this society.

1. M. McDonald. Saturday Review. Feb. 2, 1980. p. 13-16.
2. E.O. Wilson. Sociobiology: The New Synthesis. Cambridge, MA Harvard University Press. 1975.
3. S.L. Chorover. From Genesis to Genocide. MIT Press, Cambridge, MA. (1979).
4. A.R. Jensen. Harvard Educational Review. 39, 1-123 (1969).
5. R. Herrnstein. The Atlantic Monthly. 228, (3) 43-64 (1971).
6. E. Allen et al. The New York Review. Nov. 13, 1975.
7. E.O. Wilson. The New York Review. Dec. 11, 1975.
8. For review, see J. Alper, J. Beckwith and L. Miller. In The Sociobiology Debate. ed. by A. Caplan. Harper and Row. NYC. 1978 p. 476. Also in the Sociobiology Packet. Science for the People.
9. ———— Time Magazine. Aug. 1, 1977. p. 54.
10. ———— Business Week. Apr. 10, 1978.
11. E.O. Wilson. On Human Nature. Harvard University Press. 1978.
12. D.P. Barash, Sociobiology and Behavior. N.Y.C. Elsevier (1977).
13. S. Morris. Playboy. Aug. 1978. p. 108.
14. D.P. Barash. op. cit.
15. D. Freedman. Human Sociobiology. (1979).
16. The source for much of this information is M. Billig. Psychology, Racism and Fascism. A.F. and R. Publications, 21, Great Western Building, 6 Livery St., Birmingham, U.K. Available from Science for the People office.
17. N.Y. Times of either Nov. 14 or 15, 1979.
18. J. Brunn. La Nouvelle Droite. Nouvelles Editions Oswald. Paris (1979).
19. D. Dickson Nature 282, 348 (1979).
20. ———— Nature 282, 435 (1979).
21. ———— Time Magazine. Aug. 13, 1979. p. 31.
22. J. Kandell. The New York Sunday Times. July 8, 1979. p. 3.
23. F. Lewis. N.Y. Times. Sept. 26, 1979. p. A5.
24. A. DeBenoist. Le Figaro Magazine. Saturday, June 30, 1979. p. 61.
25. K. Evin. Le Nouvel Observateur. July 2-8, 1979. p. 32.
26. ———— Le Point. August 20, 1979. p. 33.
27. J. Ruffie. Le Monde. Sept. 11, 12, 1979.
28. ———— Nature. Nov. 29, 1979, p. 435.
29. Quoted in T. Sheehan. The New York Review. January 24, 1980, p. 15.
30. Y. Christen. Le Figaro Magazine. March 1-7, 1980. p. 72-76.